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THE SO-CALLED ELEMENTS.

By Prof. C. A. Young.

MY own spectroscopic observations bearing on the elementary constitution of matter have not been very numerous or important, as compared with those of several other investigators; at the same time, they have had a place in the discussion of the subject, and I cheerfully comply with the Editor's request to present a short account of them to the readers of Knowledge.

The idea that our so-called chemical elements are not really and absolutely elementary, but are built up either of atoms of some one universal matter, or of some few substances of a higher order of simplicity than themselves, is neither improbable nor new. As Mr. Lockyer points out, Dumas in 1836, and Brodie in 1867, not to mention others, have expressed themselves very clearly in this direction.

But the present special interest in the question is due mainly to Mr. Lockyer himself, who, in 1873, by an able and extensive induction from all available spectroscopic observations, put the theory in a new light, and brought together in its favour a great body of evidence, to which he has since then been continually adding.

In the discussion of the subject, certain observations of my own have been made use of along with those of others, always, I believe, with due and courteous acknowledgment, and in a manner to which I have nothing to object, unless, perhaps, that too much weight has sometimes been given to certain numbers which I had assigned only provisionally, and on a very short experience.

These observations of mine were for the most part made in July and August, 1872, during a six weeks' stay at Sherman, the summit station of the Union Pacific Railroad, at an elevation of 8,300 feet above the sea level. The party was connected with the organisation of the Coast Survey, and the results of the observations are published in the "Coast Survey Report" for 1872.

The work consisted mainly in a record of the bright lines observed in the spectrum of the solar prominences, and of the lines which were widened or otherwise modified in the spectra of sun-spots. The resulting catalogues contained respectively 273 and 156 lines, but only that portion of the spot spectrum lying between *B* and *b* was included. In these catalogues an attempt was made to assign numbers indicating the relative intensity of the lines and their proportional frequency of appearance; but, as has been intimated, the number of observations was far too small to make such figures very reliable.

Several remarkable facts were, however, very clearly brought out. In the first place, those lines of the spectrum of any particular substance (iron, for instance) which are most conspicuously and frequently seen, as *bright* lines in the spectrum of the chromosphere and prominences, are not generally the same as those most notably *widened* in the sun-spot spectrum; nor are either of these the ones which are most prominent in the ordinary solar spectrum or in the spectrum of the electric arc. Each of these four spectra selects and emphasizes, so to speak, its own set of so-called iron lines, and the same is true of other substances. Second: Where distortions and disturbances of the lines indicated rapid motions of the solar gases along the line of sight (approaching to, or receding from, the observer), it frequently happened that neighbouring lines, due to different substances, were differently affected; certain lines of iron might be violently displaced, while the adjacent lines of calcium were not disturbed at all. In some instances, also, it was noticed that lines, given in the maps as belonging to the same element, and actually in the same field of view, behaved very differently; one of them indicating violent motion of the gas concerned, the other showing nothing of the kind. But this observation seemed at the time so strange, and the instances were so few, that, in view of the possibility of mistake, it was not thought best to publish it without further confirmation, which it has since abundantly received.

In the third place, and most important, it was found that a very surprising proportion of the lines conspicuous in the spectra of the chromosphere and sun-spots are lines which on the maps are given as common to the spectra of two or more elements—lines which Mr. Lockyer has since designated by the term "basic." Nearly one-third of all the lines in the two catalogues belong to this class, while in the ordinary solar spectrum, they amount to less than four per cent. of the whole.

At that time the prevailing opinion, I think, was that these common lines were due to impurities in the specimens of the metals employed in mapping the spectra. If, for instance, in making a map of the spectrum of iron, the bits of iron used for the electric spark to play between (technically the "electrodes") were slightly contaminated with calcium, then we should expect the more important lines of calcium to appear faintly in the map of the iron spectrum; and the difficulty of getting perfectly, or even "spectroscopically," pure iron is enormous.

It was evidently possible also that these coincidences might be such only in appearance—cases of accidental juxtaposition too close for resolution by the existing spectroscopes.

But the instances were so numerous, that, on the whole, it looked more probable that the true explanation of such coincidences would be found in something common to the elements concerned; and at the time, I thought a similarity of molecular structure more likely than a community of substances. It appeared very possible that metals so closely alike in many of their properties as iron and chromium, for instance, might owe this general resemblance to such a likeness of their molecular architecture as would also carry with it certain identical vibration-periods. If atoms are vortex-rings in a continuous medium, the likeness might be in the size and configuration of the rings; if, on the other

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hand, we are to think of the ultimate atoms as little hard balls, all exactly alike, and the chemical (elementary) molecules as aggregations of a greater or less number of these atoms, variously arranged, it would be easy to imagine some such likeness in the molecules of allied substances as would account not only for the spectroscopic facts, but many others.

Mr. Lockyer, however, after experimentally disposing of the theory that these "basic" lines are due to impurities, has preferred the hypothesis that what is common to two elements which show certain identical lines in their spectra, is some *constituent substance*, found in both, and liberated in greater or less proportion with change of temperature; and he finds in the existence and behaviour of these lines one of his strongest arguments in favour of the compound character of the chemical elements.

But I fear that the foundations of this argument are insecure, however it may be with the other evidence upon which he rests his case.

My observations in 1872 were made with a spectroscope inferior to very few then in use. It had a dispersive power equivalent to that of a train of twelve flint-glass prisms of 60°, with collimator and view-telescope of 10 in. focal length. In optical perfection I have never seen, and do not expect to see, its superior; of course, it easily showed every line laid down upon the maps of Kirchhoff and Angstrom, and a multitude beside.

But soon after this time Mr. Rutherfurd's diffraction gratings began to come into use; and as they were made of continually-increasing fineness and excellence, something like a new world opened before observers in respect to the details and structure of the solar spectrum. Up to 1877, no gratings were made with a ruled surface exceeding about one square inch in area; but in that year Mr. Chapman (Mr. Rutherfurd's mechanician), at my request, and with Mr. Rutherfurd's hearty concurrence, altered the machine so that it could draw a line $1\frac{3}{4}$ in. long, and ruled for me three plates on speculum metal, with diffraction surfaces of nearly four square inches. One of these new plates allows the use of so much larger object-glasses, that, by giving a suitable focal length to collimator and telescope, the available dispersion is increased four-fold over that obtainable from the smaller plates, with the same apparent brightness of the spectrum. Perhaps I may mention in an aside that one of our most distinguished American physicists has now in hand a machine, nearly completed, and bidding fair to be perfectly successful, which rules lines four inches in length. With this he expects to make gratings having a ruled surface 4 in. by 6 in., and with 20,000 lines to the inch, or even finer. *Quod faustum sit.* One would be almost ready to die after a good study of the solar spectrum with such a grating and accessories to match. But with one of Mr. Chapman's best gratings, $1\frac{3}{4}$ in. by $2\frac{1}{2}$ in., containing over 40,000 lines (17,300 to the inch), combined with collimator and telescope of 42 in. focus and 3 in. aperture, and a magnifying power of 250 (which is perfectly borne under the best conditions, when the grating is in good humour, perfectly flat and of uniform temperature throughout)—with this instrument, I say, one reaches about the limit of present abilities. Hundreds of lines shown as single in the older maps of the solar spectrum turn out to be double, triple, or multiple, and the vacant spaces of the spectrum are filled with crowds of fine lines and details of shading before undreamed of.

When attacked with this instrument, nearly every one of the "basic" lines of the solar spectrum is resolved; out of seventy such on Angstrom's map, only seven withstand its power, and three of these seven are probably on the "basic" list by mistake, as the numerical tables of Thalen are at variance with the map regarding them. With an instrument almost identical with my own, Professors Living and Dewar have recently investigated the electric-arc spectra of nearly all the metals involved. Their results accord with mine in almost every instance. They find that the apparent coincidences between the lines of different substances always break down under powerful dispersion; so that as matters now stand, I see no evidence to be drawn from such coincidences in favour either of Mr. Lockyer's view, or of the slightly-different hypothesis which I advocated myself.

The spectra of different metals, so far as we can now make out, have absolutely no points of exact agreement.

As to the remarkable and disproportionate number of these apparently common lines which are found in the catalogues of chromosphere and sun-spot spectra, may not the explanation be substantially as follows:—A line which is really composed of two or more belonging to different elements is much more likely to catch the eye than others. In the first place, this line will appear when either of the metals vigorously reverses its lines, even though the other does not; and again, in cases where both metals reverse their lines, but too feebly to be detected against the background of the atmospheric spectrum, this line, of double brightness, will be clearly seen. Since, probably, all, or pretty nearly all, of the lines are actually reversed close to the sun's limb, though only a few are usually bright enough to be caught by ordinary instruments under ordinary atmospheric conditions, it is easy to see that a slight advantage of the kind indicated above would give a composite line a great lift in the scale of relative frequency and brightness.

I have not yet been able to apply the highest obtainable dispersion to the examination of the spots and prominences, as the great spectroscope is too large to be attached to the eye-end of our 12 ft. equatorial. I have, however, used the same grating with collimator and telescope of 12 in. focus, giving about one-fourth the resolving power of the large instrument. Even this is more than four times as powerful as the prismatic spectroscope used at Sherman, and with it, it becomes perfectly clear that the catalogues of prominence and sun-spot lines contain many serious errors, the correction of which will be likely to remove some puzzling anomalies. In a number of cases, lines which are noted in the catalogue as bright in the spectrum of a prominence, or thickened in that of a spot, turn out to be entirely unaffected, the real culprit being a neighbouring line, so fine and so close to the larger one, as to be absolutely indistinguishable from it with the old instrument.

When our great equatorial (of 23 in. aperture, and 30 ft. focus) is mounted, as it will be next spring, it will be able to carry the large spectroscope without difficulty, and I shall then hope to review the catalogues with power enough to settle most of the questions of this sort. Until such a review is made, it seems to me that generalisations founded on our present data must be very cautiously handled.

WHAT IS A GRAPE?

By Grant Allen.

THEY make a beautiful picture, these big English hot-house black Hambros, with their purple bloom and their waxlike texture, clustered thickly together in rich luxuriance on their slender and heavily-weighted branching fruit stalks. Indeed, we have now cultivated them to such a pitch of excellence, that their old wild ancestors would hardly recognise them to-day for members of the same

original woodland family of Oriental climbers. Yet, after all, we have only been able to carry a little further, by careful selection and tillage, the peculiarities which Nature had long since produced in the primitive native stock. At best, man can only develope more fully what the plant itself has well begun. Our ornamental flowers are but the handsomest chosen wild blossoms; our cereals and edible roots are but the starchiest wild seeds and tubers; and our garden fruits are but the pick of the hips and haws and wayside berries, improved and altered by ages of cultivation.

The grape-vine, they say, comes to us originally from the shores of the Caspian. Even in its native condition it produces little sweetish acid grapes, hanging in purple clusters among its green festoons. The question is, then, of what use to the plant itself are these juicy fruits? For we now know that whatever use man may make of this, that, or the other organ in any particular plant or animal is, so to speak, an accidental after-thought; the organ always subserves besides some useful purpose in the economy of the plant or animal itself to which it belongs. Now, of course, the main use of all fruits is to produce or contain the seeds. They are merely seed-vessels, and, in most cases, they are dry and brown when ripe, like the pea-pod, the poppy-head, or the capsule of the mignonette. The problem we have to answer in the case of the grape is therefore this: Why should it be pulpy and prettily coloured, while these other fruits—and, indeed, the vast majority of all fruits—are mere dry and unattractive organs?

The analogy of red and white and yellow flowers affords us a good hint towards the solution of this problem. We know that flowers have acquired their bright hues, their honey, and their perfume, for the sake of attracting the insects which fertilise them by carrying pollen from head to head. Is there any way in which fruits can similarly benefit by alluring the eyes of any animal race? At first sight this would seem impossible; but a little consideration will show us a way out of the difficulty. Most plants, it is true, can only lose by allowing their seeds to be perceived and eaten by animals. In such cases the fruit, be it pod or capsule, is usually inconspicuous in colour, and drops its tiny little seeds quietly out upon the ground beneath. Those plants which best succeed in diverting the attention of seed-eating birds or mammals from their fruits, outlive, in the long run, their less adapted neighbours; and so the survival of the fittest has brought it about that ninety-nine kinds out of a hundred in our own day have unnoticeable little green or brown seed vessels, such as those of the chickweed, the pimpernel, and the clover tribe, which nobody but a botanist ever observes at all. Suppose, however, that any plant happens to have its seeds covered with a moderately hard and indigestible outer coat, would it not then be rather benefited than otherwise by having these seeds enclosed in a soft and juicy bed of edible pulp? For in that case birds and other animals might eat the seeds, fruit and all, for the sake of the pulpy covering; and as the hard little shell would protect the young embryo within, this vital part would not be digested, but would pass uninjured through the creature's body. By such an arrangement the plant would not only get its seeds dispersed—in itself a most important matter—but would also have the young seedling well manured and started in life under unusually favourable auspices. If such a tendency were ever to be set up even in the slightest degree by a mere sport or chance variation, we may be sure the variety in which it appeared would be so favoured by circumstances, that it would soon become a marked and distinct species.

As a matter of fact, it is pretty certain that such has been the origin of all edible pulpy fruits. Take, for example, these grapes here. If you cut one of them open, you will find inside a number of hard little seeds. Slice one of these again with a sharp penknife, and you will see that it consists of a tiny embryo plant in the centre, surrounded by a very solid bony shell. Each seed is in fact a miniature nut; and the kernel, so to speak, consists of the tiny plantlet within, together with the albumen on which it feeds when it first begins to germinate. Now, if any bird were to swallow and digest this vital part of the seed, the plant would, of course, be an obvious loser. But the hard shell prevents such a catastrophe from happening; and, therefore, the plant is benefited by the soft, eatable pulp which surrounds these little mimic nuts. Observe, too, that the fruity part of the grape is sweet; it contains grape sugar. Now sugar is always laid up in those parts of plants which specially seek to attract the animal world. In flowers, the nectar allures the fertilising bees and butterflies; in fruits and berries, the sweet juices allure the birds which disperse the seeds; nay, even the pitcher plants secrete honey to wile flies into their insect-eating cups; and certain acacias store it up in hollow thorns to attract the epicurean ants, which, in turn, protect the tree by driving away their leaf-eating relations. In almost every case, one may say that where sugar is found in any organ of a plant, it is placed there for the sake of engaging the attention of some animal ally; while conversely, all flower-feeding and fruit-eating creatures always manifest a marked taste for sweet substances, dependent upon their long habituation to sugary food.

Not only, however, are the grapes sweet, but they are also brightly coloured. Naturally, among succulent fruits bidding for the attention of birds, those would best succeed which were most visible at some little distance. Accordingly, just as the insect-fertilised flowers have developed brilliant pigments in their petals, so the fruits which depend upon birds for the dispersion of their seeds have acquired prettily-coloured coverings. We all know how noticeable are the hips and haws, the holly-berries and rowan-berries, even among our northern woodlands; while the oranges, mangoes, and pomegranates of the tropics appeal even more vividly to the sharp eyes of monkeys, parrots, and toucans. At the same time, it is noteworthy that the tastes of birds with regard to colour seem to differ somewhat from those of insects; for, as Mr. Wallace points out, white, which is a common colour for flowers, is rare among fruits; while purple and bluish-black, which are seldom met with among flowers, may almost be considered as the ordinary colours of most wild fruits.

Looking closely into my cluster of grapes, again, I see that it still contains two or three unripe and stunted specimens. These, of course, are pale-green, like the leaves, and when I taste one of them I find it unpleasantly harsh and acid to the palate. This reminds me that grapes, like other fruits, are not at all stages of their existence sweet and brightly coloured. While the seeds are still immature, they would only lose by being eaten, because they are not yet fit for germination. In this stage, therefore, the skin is filled with green colouring matter, and the cluster is quite inconspicuous among the foliage which surrounds it. It does not *want* to attract attention in its present stage. Furthermore, the pulp at this period is filled with tartaric acid and other sour juices, to repel any too-inquiring or too-impatient visitor. But as the seeds mature, the fruit ripens—that is to say, a chemical change goes on in the pulpy portion, which results in the formation of grape-sugar. At the same time, other chemical changes taking place in the skin result in the production of the

purple bloom, which advertises to the birds the presence of the sweet juice within. The whole process obviously aims at concealing the fruit and rendering it unpalatable while the seeds are immature, and at making it conspicuous as well as pleasant the moment the seeds are ripe for dispersion. Hence we are justified in concluding that the development of the grape is due to the long selective action of fruit-eating birds. Originally, no doubt, the primitive ancestral vine produced smaller and harder seed-vessels, which were probably green when young, and brown when ready to fall upon the ground. But some of them happened to show a tendency towards producing larger and juicier fruits, and these were constantly favoured by the unconscious friendliness of the neighbouring birds. The colour and the sweetness would soon follow, as they have followed a thousand times over in the development of each separate edible fruit. A grape, in short, viewed from the standpoint of the vine itself, is merely a cunning device for ensuring the assistance of birds or mammals in dispersing the little, nut-like seeds of which man takes, as a rule, such scanty notice.

STUDIES OF VOLCANIC ACTION.

By G. F. Rodwell.

PART II.

OF late years the microscopic study of eruptive rocks has revealed many facts of importance. The most opaque black lavas and basalts are seen to be mainly made up of colourless transparent crystals, when a slice less than a hundredth of an inch in thickness is placed under the microscope. Polarised light enables us to distinguish the nature of the crystals, and the angles can be readily measured. Zirkel, Rosenbusch, and Rutley have done much to promote this branch of petrology. One of the most recent and beautiful works on the subject is the "Minéralogie Micrographique" of MM. Fouqué and Michel Levy, which contains more than fifty coloured quarto plates of rock sections seen under the microscope, usually by polarised light. The rocks are, for the most part, eruptive. A peculiarity of the book is an ingenious plan for recognising in a moment the different mineral constituents of the section; for this purpose each plate is covered by a loose piece of transparent paper upon which an uncoloured outline of the engraving is drawn, and each mineral has its own number placed within the outline. The same number is used throughout the book to designate the same mineral.

The frontispiece of Prof. Judd's "Volcanoes" shows six beautiful sections of eruptive rocks, in which the passage from the perfectly glassy to the highly-crystalline structure is strikingly illustrated. The first specimen is that of a volcanic glass or obsidian, a lava which was rapidly cooled from a condition of complete fluidity, and which shows nebulous patches scattered through a glassy base. A very high power reveals that these patches are composed of minute crystals, called *microliths* or *crystallites*; and we are forcibly reminded of the resolution of the heavenly nebulæ into thickly-clustered stars by the microscope's twin-sister. Sometimes the microliths are built up into germ-like forms within the ground mass of the lava; and again, as the crystalline forces come more and more into play, the microliths form radial groups about definite centres, and thus build up globular masses, called "sphærulites." Crystals are made up of microliths grouped about certain axes, and a completely amorphous glassy lava may be converted into a highly-crystalline mass by slow cooling, and thus giving the molecular forces time to act in the grouping of the microliths. Most lavas, when viewed under the microscope, exhibit a glassy paste or ground-mass, containing microliths, among which distinct crystals are distributed. Or again, when lavas consolidate at a great depth beneath the surface, the ground-mass is made up of small crystals, through which larger crystals are distributed. And, finally, we arrive at the granitic structure, in which the rock is completely made up of large crystals without any ground-mass. One and the same rock may exist in each of these forms, according as it has been cooled slowly or rapidly, at a great depth beneath the surface, or near to it. Thus, while *basalt* represents the lava form of a volcanic rock, *gabbro* is its granitic or crystallised form, and *tachylyte* its glassy or obsidian form. They all have the same ultimate chemical composition.

The larger crystals probably separated from the amorphous masses beneath the volcano, and were carried up to the surface by the fluid material forming the ground mass of the lava. Crystals frequently furnish abundant evidence of having been formed under enormous pressure. When examined by high powers of a microscope the crystals of granitic rocks are sometimes seen to contain cavities filled with liquid or gas, or with two liquids and a gas. Sometimes the liquid is water, sometimes a hydrocarbon like the mineral oils which are found in abundance in deep-seated rocks in various parts of the world. Not unfrequently the inclosed substance is liquefied carbonic acid, a gas which requires a pressure of nearly 600 lb. on the square inch to liquefy it at the freezing point of water, and a much higher pressure at the temperature which exists a short distance beneath the surface of the earth. We have thus every reason for believing that the crystals of a rock have been formed in deep recesses in the earth, while the ground mass has solidified at the surface.

Of recent volcanic outbursts, there can be no doubt that the eruption of Etna in May, 1879, was the most considerable. It was studied very fully by Professor Silvestri, who considers that it was the fulfilment of the abortive attempt made by the volcano in 1874. On the 29th of August of that year a rift opened on the N.E. side of the mountain between the crater and Mojo, and thirty-five monticules were thrown up along its course, with one large crateriform mound from which lava was discharged. After seven hours of activity the dynamic forces suddenly decreased, and in two days all the effects ceased, but the rift remained open, and earthquakes were common in the vicinity. Silvestri then predicted that when the next eruption occurred the rift would prove the line of least resistance, and that lava would flow from it, and craters be opened along its course, and this prediction was completely verified in the eruption of 1879. The fissure then extended itself to a length of six miles, passing through the great crater. Eight eruptive mouths opened on the south side of the mountain, and discharged a small amount of lava; but the lava presently found an exit at a lower level on the north side, and on May 28 Silvestri observed a great column of smoke, soon followed by the gloomy leaden tint observed during an eclipse, and by showers of volcanic sand. In ten minutes he collected more than two pounds of this sand in an inverted umbrella. He then approached nearer to the scene of action, and, when about 6,200 ft. above the sea, he heard loud detonations and experienced considerable oscillations of the soil. As he approached the great rift, he noticed three new craters near Monte Nero, from which issued dense clouds of steam. From one of them lava flowed which formed a considerable stream, reaching to a distance of 6½ miles by the evening of the 29th. It stopped about

half a mile from the village of Mojo, and then had a breadth of 23 ft. and a height of 32 ft. When the lava stream was examined near its source by the spectroscope, it showed the lines of hydrogen, calcium, sodium, and potassium. Silvestri has embodied his observations in a monograph; he has also just finished an important work, illustrated by photographs showing various phases of eruption of Etna. The work will no doubt be in print before the end of this year.

From Sicily, we pass at one stride to that country of which the Danes say: "God made the rest of the world; the devil made Iceland." A few months prior to the outburst of Etna, Hecla, which had not been in eruption since 1845, showed signs of disturbance, and threw up a hillock about four miles to the N.E. of the Great Crater. A large quantity of very pumiceous lava was emitted, and, at the time when the writer visited it, great quantities of hydrochloric acid were discharged from crevices in the lava, but the eruption was otherwise unimportant.

Since 1872, Vesuvius has been only active at intervals. Palmieri has published a full account of the 1872 eruption, and in annual reports has given the history of the mountain since that date. The eruption of Santorin, which began in January, 1866, and lasted till October, 1870, has furnished results of great interest to vulcanologists, and it has led to the publication of M. Fouquè's magnificent "Santorin et ses Éruptions," in which the whole history of the volcanic phenomena and the products is given. The mineralogical results have been of especial interest. Also we may note the peculiarity that, in the most violent period of activity, inflammable gaseous exhalations, which took fire on coming into contact with red-hot lava, were emitted. The flames, when examined by the spectroscope, were found to consist of hydrogen, containing small quantities of copper, sodium, and chlorine. As the gaseous emanations of Santorin are emitted under water, they are not burned at the point of issue, and can hence be examined readily. As much as 56 per cent. of hydrogen was found in some of the exhalations, and the author concludes, we think without sufficient warrant, that the enormous quantities of water-vapour emitted in volcanic eruptions exist in the red-hot magnia of lava, not as water, but as dissociated hydrogen and oxygen.

The record of recent earthquakes will be fresh in the minds of our readers. Three calamitous earthquakes have occurred within the last eight months—at Ischia, in Chios, and at Agram. In the latter 4,000 persons perished, double that number were maimed, and 30,000 were rendered homeless. It commenced on April 3, but several months afterwards a minaret and a tottering wall were overthrown, and in all there were at least 250 shocks. In 1870, a great earthquake occurred in Phocis, north of the Gulf of Corinth. Professor Julius Schmidt, the Greek Astronomer Royal, has given a detailed account of it. It is asserted that during the first three days a shock was felt every third minute, while Schmidt himself counted nearly 2,000 in the twenty-four hours, four days after the great outbreak. Altogether, during five months of 1870, it is calculated that the shocks and detonations exceeded half-a-million. In October, 1870, the most severe shock of earthquake observed during the present century in the north-eastern states of North America was felt at Quebec. It was instantly telegraphed to Montreal, and the message arrived nearly half a minute before the shock. The Peruvian earthquake of Aug. 13, 1868, was felt the same evening in Hawaii, 6,300 miles distant.

We are as far as ever from the knowledge of the cause of earthquakes, but, undoubtedly, high pressure steam has much to do with it. The author of the article, "Earthquakes," in the July *Quarterly Review*, propounds a theory to the effect that electricity is the true cause, but in what manner he has not clearly defined. We do not consider that his arguments will bear criticism. Here is one of them:—"The vicinity of hot springs, volcanoes, mud lakes, regions of intense heat, and centres of electric influence, are the special haunts of earthquakes, and science has pretty well proved that heat and electricity are convertible."* One word in conclusion as to volcanic products. But few new minerals have lately been found. Professor Scacchi announced two years ago the discovery of a new element, which he termed *vesbium*, among the lavas of Vesuvius, but we have not heard that it has ever been isolated. The most remarkable volcanic product lately discovered is undoubtedly a substance from the crater of Volcano, analysed by Professor Cossa, of Turin. It was found to contain seven non-metals and eight metals, among the latter the rare and recently-discovered elements, cæsium, rubidium, and thallium.

OUR UNBIDDEN GUESTS.

By Dr. Andrew Wilson, F.R.S.E.

PART II.

IT is much the same with the tapeworm-tribe as with the fluke (considered in Part I.) The common tapeworm of man (*Tænia Solium*) consists of a very minute "head," attaching itself by suckers and hooks to man's intestines; of a slender "neck," and of hundreds of "joints." Each "joint" is really a semi-independent *animal*; and the tapeworm is therefore a *compound* animal, and presents us with a *colony* of similar beings. A large tapeworm may measure 20 or 30 ft.; and new joints are continually being "budded" out from the head and neck. Hence the physican can never be sure that he has cured a case of tapeworm until he has seen the head and neck of the animal. If a man swallowed the *egg* of a tapeworm, he would not be infested thereby. The young worm has to pass its early life in the body of another warm-blooded animal; and in the case of the common tapeworm, it is "the gintleman that pays the rint," which acts the part of nurse or first host. Man, in other words, obtains his common tapeworm guest from the pig. When this animal swallows the egg of a tapeworm, the young worm bursts through the egg-case and bores its way to the pig's *muscles*. If the porker is affected by numerous embryos, that is, if it has swallowed a large number of eggs, it will become feverish and ill, and it will then be said to have developed "measles." The "measles" of the pig are the visitations of young tapeworms. In the muscles of the pig, then, these young worms rest. *Je suis ici; j'y reste*, is decidedly the motto of the young worm. It developes a little head and neck, and it also, by way of a tail, produces a little bladder or bag. Before naturalists knew its true nature, it was regarded as a special kind of parasite, and was named a "cystic worm."

If the pig dies a natural death and is respectably interred, or if the pig should live long enough, these youthful tapeworms will respectively perish, or will degenerate and disappear from the tissues of the aged porker. But assuming that the usual Nemesis of the pig race overtakes the animal, then, in the form of pork, it will gladden the heart of certain members of the human race. Now, let us suppose that a man eats a portion of the "measly pork." Let us further suppose that the pork has been

* The article is utterly without scientific value. How it found ts way into a magazine of good position is a mystery.—Ed.

imperfectly cooked; then comes the "tide of fortune" to the young worms. For when the young worm has been eaten by the man, the bladder-tail drops off. Each little head and neck finding itself in the human stomach, recognises its lawful *habitat*. Each attaches itself to the lining membrane of the human intestines, and each by a process of budding produces joint after joint, until man is presented with his matured "guest."

If we tabulate matters thus, the history of the tapeworm will become clear:—

1st Epoch.	Stage 1. The egg derived from parent tapeworm of man.	Passed in the pig as host.
	„ 2. Swallowed by the pig; developing.	
	„ 3. The "Resting Larva," or cystic worm, in the pig's muscles, and forming "measly" pork.	
2nd Epoch.	„ 4. Swallowed by man.	Passed in man as the host.
	„ 5. Development of the head and neck, and attachment to man's intestine.	
	„ 6. The production by budding of the adult worm.	

As a last piece of parasite biography, we may glance at the history of a form which now and then attracts the notice of even Imperial Parliament itself. This form is the famous *Trichina*, which acquires an unenviable notoriety, in that it may, unlike the tapeworm race, cause the death of its "host." Each trichina is a minute worm, coiled, in its immature condition, within a little sack or bag, which in numbers may be found again in the muscles of the pig. Where the pig gets its trichinæ from is hardly certain, but rats are believed to be the sources of supply for the pig race. In the muscles of the pig each trichina is, as already remarked, in an imperfect and youthful condition. If the pig lives long enough, the trichinæ will disappear from its muscles, or will degenerate to become mere specks of lime. But should a man eat a portion of trichina-infected pork, the youthful worms will undergo, in his digestive system, a rapid and extraordinary development. The trichinæ will develope enormous numbers of young; and the young brood will now naturally seek the muscles of man for a habitation, as their parents before them sought those of the pig. Then ensues the tug of war for the afflicted human. The pain caused by the boring of these microscopic worms from stomach to muscles is intense. It is this pain and attendant symptoms that constitute the disease known as trichiniasis. If the patient's strength holds out, he is safe whenever the young trichina-brood enter the muscles. There they rest, and remain to degenerate—unless, indeed, cannibal instincts were represented in the human race; in which case the cannibal world experience a few of the tortures and troubles which are said proverbially to afflict the just and unjust alike.

The great lesson to the learned, from our survey of parasites, is care in the choice and increased care in the cooking of our food. It should be remembered that the germs of these parasites are killed by a sufficiently long exposure to heat. Hence, whilst underdone meat may have its charms, it has likewise its grave dangers. Pork, in any and every fashion, should at all times be thoroughly cooked. In this latter case, the parasitic horde may not merely be destroyed, but may even contribute in a microscopic way, to human nutrition.

Mr. S. L. M. Barlow, one of the trustees of the Metropolitan Museum of Art, New York, has presented to that institution, through its director, General di Cesnola, a collection of vases lately discovered in an Egyptian tomb supposed to date before the Exodus.

SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART V.

As already stated, in Part 3 of this series, page 88, the conversion of water into steam under ordinary atmospheric pressure demands 966·6° of heat over and above that which does the work of raising the water to 212°, or, otherwise stated, as much heat is at work in a given weight of steam at 212°, as would raise the same quantity of water to 1,178·6° if it remained liquid.

James Watt concluded from his experiments that a given weight of steam, whatever may be its density, or, in other words, under whatever pressure it may exist, contains the same quantity of heat. According to this, if we reduced the pressure sufficiently to bring down the boiling point to 112°, instead of 212°, the latent heat of the steam thus formed would be 1,066·6° instead of 966·6°, or if, on the other hand, we placed it under sufficient pressure to raise the boiling point to 312°, the latent heat of the steam would be reduced to 866·6°, *i.e.*, only 866·6° more would be required to convert the water into steam. If the boiling point were 412°, as it is between 19 and 20 atmospheres of pressure, only 766·6° more heat would be required, and so on, till we reach a pressure which raises the boiling point to 1,178·6°, the water would become steam without further heating, *i.e.*, the critical point would be reached, and thus, if Watt is right, we can easily determine, theoretically, the critical temperature of water.*

Mr. Perkins, who made some remarkable experiments upon very high pressure steam many years ago, and exhibited a steam gun at the Adelaide Gallery, stated that red-hot water does not boil; that if the generator be sufficiently strong to stand a pressure of 60,000 lb. load on the safety valve, the water may be made to exert a pressure of 56,000 lb. on the square inch at a cherry red-heat without boiling. He made a number of rather dangerous experiments in thus raising water to a red-heat, and his assertion that red-hot water does not boil is curious when viewed in connection with Dr. Andrew's experiments.

I cannot tell how he arrived at this conclusion, having been unable to obtain the original record of his experiments, and only quote the above second-hand. It is worthy of remark that the temperature he names is about 1,170°, or that which, if Watt is right, must be the critical temperature of the water. Perkin's red-hot water would not boil, being then in the intermediate condition.

So far, we have a nice little theory, which not only shows how the critical state of water must be reached, but also its precise temperature; but all this is based on the assumption that Watt made no mistake. Unfortunately for the simplicity of this theory, Regnault states that *his* experiments contradict those of Watt, and prove that the latent heat of steam does not diminish just in the same degree as the boiling-point is raised, but that instead of this the diminution of the latent heat progresses 30½ per cent. more slowly than the rise of temperature, so that, instead of the latent heat of steam between boiling-points of 212° and 312° falling from 966·6° to 866·6° it would only fall to 895·1° or 69·5° for every 100°.

If this is correct, the temperature at which the latent heat of steam is reduced to zero is much higher than 1,178·6°, and is, in fact, a continually receding quantity never absolutely reached; but I am not prepared to accept these figures of Regnault as implicitly as is now done in text

* Watt's own figure for the latent heat of steam at 212° was 950°, but I adopt that which is now generally accepted.

books (I was nearly saying "as is now the fashion"), seeing that they are not the actual figures obtained by his experiments, but those of his "empirical formulæ" based upon them. His actual experimental figures are very irregular; thus, between steam temperature of 171·6° and 183·2° a difference of 11·6°, the experimental difference in the latent heat came out as 4·7°; between steam temperature of 183·2° and 194·8°, or 11·6°, again the latent heat difference is tabulated as 8·0°.

Regnault's experiments were not carried to very high temperatures and pressures, and indicate that as these advance the deviation from Watt's law diminishes, and may finally vanish at about 1,500° or 1,600°, where the latent heat would reach zero, and there, according to the above, the critical temperature would be reached. Any additional heat applied after this will have but one function to perform, viz., the ordinary work of increasing the bulk of the heated body without doing anything further in the way of conferring upon it any new self-repulsive properties.

Our notions of solids, liquids, and gases are derived from our experiences of the state of matter here upon this earth. Could we be removed to another planet, they would be curiously changed. On Mercury water would rank as one of the condensible gases; on Mars, as a fusible solid; but what on Jupiter?

Recent observations justify us in regarding this as a miniature sun, with an external envelope of cloudy matter, apparently of partially condensed water, but red-hot, or probably still hotter within. His vaporous atmosphere is evidently of enormous depth, and the force of gravitation being on his visible outer surface 2½ times greater than that on our earth's surface, the atmospheric pressure in descending below this visible surface must soon reach that at which the vapour of water would be brought to its critical condition. Therefore we may infer that the oceans of Jupiter are neither of frozen, liquid, nor gaseous water, but are oceans or atmospheres of critical water. If any fish-birds swim or fly therein they must be very critically organised.

As the whole mass of Jupiter is 300 times greater than that of the earth, and its compressing energy towards the centre proportional to this, its materials, if similar to those of the earth and no hotter, would be considerably more dense, and the whole planet would have a higher specific gravity, but we know by the movement of its satellites that, instead of this, its specific gravity is less than a fourth of that of the earth. This justifies the conclusion that it is intensely hot, for even hydrogen, if cold, would become denser than Jupiter under such pressure.

As all elementary substances may exist as solids, liquids, or gases, or, critically, according to the conditions of temperature and pressure, I am justified in hypothetically concluding that Jupiter is neither a solid, a liquid, nor a gaseous planet, but a critical planet, or an orb composed internally of dissociated elements in the critical state, and surrounded by a dense atmosphere of their vapours, and those of some of their compounds, such as water. The same reasoning applies to Saturn and the other large and rarefied planets.

The critical temperature of the dissociated elements of the sun is probably reached at the base of the photosphere, or that region revealed to us by the sun-spots. When I wrote "The Fuel of the Sun," thirteen or fourteen years ago, I suggested, on the above grounds, the then heretical idea of the red-heat of Jupiter, Saturn, Uranus, and Neptune, and showed that all such compounds as water must be dissociated at the base of the sun's atmosphere, but being then unacquainted with the existence of this critical state of matter, I supposed the dissociated elements to exist as gases with a small solid nucleus or kernel in the centre.

Applying now the researches of Dr. Andrews to the conditions of solar existence, as I formerly applied the dissociation researches of Deville, I conclude that the sun has no nucleus, either solid, liquid, or gaseous, but is composed of dissociated matter in the critical state, surrounded, first, by a flaming envelope due to the recombination of the dissociated matter, and outside of this another envelope of vapours due to this combination.

THE EVOLUTION OF MAN.

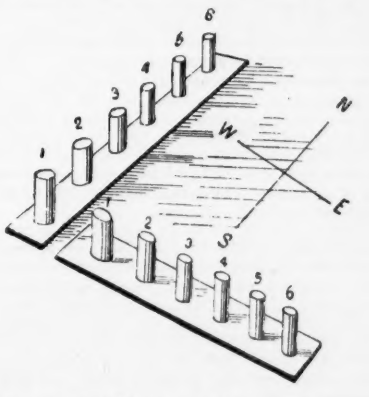
By Dr. A. Wilson, F.R.S.E.

In letter 103, "G. M." asks, "has evolution effected any change in man? If so, what change? If not, why not?" These are weighty questions, and "G. M." will find difficulty in answering them. Let him remember firstly that evolution has only been studied—rather nature, in the light of evolution, has only been studied—for some twenty-five years or so. That is, of course, a mere fractional space in the history of human thought. If we refuse to admit (as science does) that man was created a perfect being, and then became degraded, there exists only another supposition—that of evolution. If man has arisen from a savage to a civilised state, that surely is evolution. We do not yet know, because such knowledge is difficult to acquire, if the human frame is subject to the same influences as those of lower animals. But there is little doubt that elevation from savagery to civilised life means and implies "evolution," and that of considerable extent. Mentally, man's evolution cannot be doubted; the ever-widening sphere of thought has sprung from small and rude beginnings, like language itself. But man's ways of life, his power of adaptation to his surroundings, and countless other circumstances, have made the facts and course of his "evolution" very difficult to trace. If "G. M." will read Tylor's "Anthropology" (Macmillan) he will find there summarised facts and phases of human life which go powerfully to answer the interesting queries he puts to Knowledge.

MALLET'S SEISMOMETERS.

The "Encyclopædia Britannica," 9th edition, "Earthquake," mentions and describes a seismometer of extreme simplicity:—

"Its construction, which is due to Mr. Mallet, will be understood by reference to the figure. Two sets of right cylinders are turned in some hard material, such as boxwood. The cylinders are all of the same height, but vary in diameter. Two planks of wood are fixed to a level floor, one having its length in a north and south, and the other in an east and west direction. The cylinders stand upright on the planks in the order of their size, with a space between each pair greater than their height, so that when one



pillar falls it does not strike its neighbour. The surrounding floor is covered up to the level of the planks with dry sand. When a shock passes, some of the cylinders are overturned, the number depending on the velocity of the wave. Suppose the shock knocks over the narrow-based cylinders 4, 5, 6, leaving 1, 2, 3 standing,

then the velocity of the horizontal component must have been greater than that needed to overturn No. 4, but not great enough to overturn No. 3."

Then follows a formula for determining approximately the velocity—which, perhaps, it may not be necessary to trouble you with.

A. T. C.

THE FAURE ACCUMULATOR.

By W. Lynd.

THANKS to Professor Sylvanus Thompson, who has just sent me the results of his latest experiments with secondary batteries, I am able to give a brief sketch of the Faure accumulator.

So far back as 1860 M. Planté constructed a secondary battery, consisting of nine cells, in each of which two long and wide strips of lead, separated by coarse cloth, were rolled together in a spiral form and immersed in dilute sulphuric acid. A few months later he modified this form by placing side by side in a rectangular box two series of lead plates, alternately connected together, each plate being about eight inches high. He recurred afterwards to the spiral form as being more convenient, but replacing the coarse cloth by narrow strips of gutta-percha. But the cells thus constructed were not ready for immediate action. Two clean lead plates give no current of their own; they are only intended to receive and store up what is sent into them from some external source; and at first, while the lead is bright, when a current is sent through the cell from some suitable source, such as three or four Grove or Bunsen cells, the separated oxygen and hydrogen gases bubble up to the surface, for the most part leaving only a very small percentage as an adherent film, and, in consequence, yielding only very transient secondary currents. The plate of lead by which the current enters is, however, attacked by the oxygen, and becomes covered by a thin layer of brown peroxide of lead, and this film, though thin, is powerfully electro-negative towards metallic lead and towards the film of hydrogen on the Kathode plate. The cell in this condition will therefore produce a current, and in so doing, the peroxide is partially reduced to the metallic condition, and assumes in its reduction a spongy or loosely crystalline texture. If now the cell be again charged, and charged in the opposite direction, the other plate of lead becomes in like manner peroxidised, while the hydrogen bubbles are less freely evolved, for the atoms of gas unite as fast as they are liberated with the oxygen of the peroxide and reduce it to the metallic condition; every time the charging current is thus reversed, the films of peroxide, as of spongy metal, become thicker, until the lead to a considerable depth is bitten into. And every such operation increases, therefore, the power of the cell to store up in this electro-chemical fashion the energy of the currents sent into it. M. Planté ascribes the process of "forming" to a sort of electro-chemical *tanning*. The first day the alternate charging should be done at intervals of a quarter to half an hour, the cell being discharged between each operation. The next day the duration of the alternate charges may be increased from a quarter of an hour to a whole hour; the day after to two hours. After repose for a week or a fortnight, the charges may last several hours; and by the end of several months, the cell will be well "formed;" after which, it should, when used, be charged in one direction only, otherwise the whole thickness of the lead plates will be bitten into, and transformed into peroxide. These magnificent researches were carried on by Planté through more than twenty years, and it seems remarkable that even in the scientific world, that gentleman's claim to the discovery of the accumulator are not duly acknowledged.

M. Camille Faure, who has been awarded by the public press the lion's share of the glory, conceived the idea of constructing a secondary battery, in which, though the tedious process of "formation" is modified and shortened, the ultimate result is the same; namely, to produce upon lead plates, immersed in dilute sulphuric acid, a coating of peroxide of lead that can readily be reduced to the loosely crystalline metallic condition.

The Faure accumulator, of which we have heard so much lately, is simply a modification of the Planté secondary battery, and is constructed as follows:—Eleven sheets of lead, of such thickness as to weigh about 2 lb. to the square foot, are cut to the size of 12 in. by 10 in., an ear-piece being burnt on at one corner. Or six sheets are taken, five of them being twice the above size, and folded double. These are painted thickly with red lead on both sides, and against each side is pressed a piece of felt, the face of which is also thickly coated with red lead, there being about 17 lb. of lead and 23 lb. of red lead altogether. These sheets are placed side by side in a water-tight case, alternate sheets being connected together by the projecting flaps. The cell is filled up with dilute acid, the total weight being about 50 lb.

When thus prepared, the cells are formed by a process of charging by means of the current of a dynamo-electric machine, the current being sent through them for six or seven days without intermission before they are ready for use. The red lead is reduced gradually on one side to the metallic state, and on the other assumes the condition of peroxide; but the cell does not attain its fast condition for some weeks. Such is a brief *résumé* of the wonderful accumulators which are destined to work a revolution in electrical science. Those who desire to gain a thorough knowledge of the theory of the secondary batteries cannot do better than purchase a copy of Professor Thompson's work on electricity and magnetism. It is published by Macmillan & Co.; the price is only 4s. 6d.

THE WYANDOTTE INDIANS.

By Miss A. W. Buckland.

AS bearing upon the subject discussed in Knowledge, Are women inferior to men? the account given by Mr. John W. Powell, vice-president of the American Association for the Advancement of Science, of the form of Government among the Wyandotte Indians, will probably be found interesting.

In the Wyandotte Government, says Mr. Powell, four groups are recognised—the family, the gens, the phratry, and the tribe.

The family is nearly synonymous with household. The head of the family is a woman.

The gens is an organised body of consanguineal kindred in the female line. "The woman carries the gens," is the formulated statement by which a Wyandotte expresses the idea that descent is in the female line. Each gens has the name of some animal—the ancient of such animal being the tutelar god.

There are four phratries in the tribe, and this division seems to be used chiefly for religious purposes, in the preparation of medicines, and in festivals and games. The eleven gentes, as four phratries, constitute the tribe. Each gens is a body of consanguineal kindred in the female line, and each gens is allied to other gentes by consanguineous kinship through the male line, and by affinity through marriage. The family or household is not a unit of the gens or phratry, as two gentes are represented in each—the father must belong to one gens, and the mother and her children to another. The civil government belongs of right to a system of councils and chiefs. In each gens there is a council composed of four women. These four women councillors select a chief of the gens from their brothers and sons, and this chief is the head of the gentile council. The tribal council is composed therefore of one-fifth men and four-fifths women.

The four women-councillors of the gens are chosen by the heads of households, themselves being women. There is no formal election, but by frequent discussion it is decided that, in the event of the death of any councillor, a certain person will take her place. When a woman is installed as Councillor, a feast is given by the gens to which she belongs, to which all the members of the tribe are invited. The woman is painted and dressed in her best attire, and the sachem of the tribe, who is chosen by the chiefs of the gentes, places upon her head the gentile chaplet of feathers, and announces in a formal manner to the multitude, that the woman has been chosen a councillor.

The gentile chief is chosen by the council women, after consultation with the other women and men of the gens. At his installation, the council women invest him with an elaborately ornamented tunic, place upon his head a chaplet of feathers, and paint the gentile totem on his face.

Meetings of the gentile council are very informal, but the meetings of the tribal councils are conducted with due ceremony. The chief of the wolf gens, who is of right the herald and sheriff of the tribe, calls the assembly to order, fills and lights a pipe, sends one puff of smoke to heaven and another to the earth. The pipe is then handed to the sachem, who, filling his mouth with smoke, and turning from left to right with the sun, slowly puffs it over the heads of the councillors, who are sitting in a circle. The pipe is then smoked by each person in turn. The sachem then explains the object of the meeting, and each person tells what he thinks should be done. If the majority agree, the sachem simply announces the decision; but if there is a tie, the sachem is expected to speak.

It is the function of government to preserve rights and enforce the performance of duties.

These rights are:—1, Rights of marriage; 2, Rights to names; 3, Rights to personal adornment; 4, Rights of order in encampments and migrations; 5, Rights of property; 6, Rights of person; 7, Rights of community; 8, Rights of religion.

Marriage between members of the same gens is forbidden. Polygamy is permitted, but the first wife remains the head of the household. A man seeking a wife consults with her mother, and the mother of the girl tries to obtain the consent of the women councillors, but their decision is usually quietly submitted to. When a mother

dies, the children belong to her sister or nearest female kin, the matter being settled by the council women of the gens. Once a year the council women of the gens select the names for the children born during the year, and the chief of the gens proclaims them at the festival. No person may change his name, but by honourable conduct he may win another.

Within the tribal area, each gens occupies a tract for cultivation. The women councillors partition the gentile land among the householders. The ground is repartitioned once in two years. Cultivation is communal, that is, all of the able-bodied women of the gens take part in the cultivation of each household tract.

The wigwam, or lodge, and all the articles of the household belong to the woman—the head of the household; and at her death are inherited by her eldest daughter or nearest of female kin. The matter is settled by the council women. If the husband die, his property is inherited by his brother, or his sister's son, except such portion as may be buried with him. His property consists of his clothing, hunting and fishing implements, and such articles as are used personally by himself. Usually a small canoe is the individual property of the man. Large canoes are made by the male members of the gentes, and are the property of the gentes.

Each gens has a right to the services of all its women in the cultivation of the soil. Each gens has the right to the service of all its male members in avenging wrongs, and the tribe has the right to the service of all its male members in time of war.

This is a short abstract of Mr. Powell's paper, as far as it relates to the status of woman among the Wyandottes, and I feel sure it will be a surprise to many to find that the despised Indian squaw holds a position so honourable; one to which her civilised white sisters may never hope to attain; but the truth is, we find here, among the Wyandotte, the survival of a social state once very widely spread, and which probably existed wherever we find kinship traced on the female side—that is, through the mother instead of the father. It was, perhaps, the earliest of all forms of government, especially among people of Mongoloid affinities. Among the Semites, the government was patriarchal and paternal; but even among the Hebrews, we find prophetesses, possessed of no small amount of authority. It remained for the Aryan races to deprive woman of every shred of real power, whilst professing to treat her with chivalrous deference, and it is to this probably that we may attribute the diminution in the size of the heads of women in modern times, as compared with those of men—a difference which is not to be found in the more ancient skulls, as the late Professor Rolleston and other able anatomists have so often pointed out; and which it might be safely affirmed would not be found among the Wyandotte councillors; for there can be little doubt that the brain develops by use, and that in a tribe or nation wherein the burden of government is divided between the sexes, there will be no disparity in the brain power of men and women, nor in the skulls wherein those brains are contained.

NOTE ON THE SPHEROIDAL STATE.

By W. F. Barrett, F.R.S.E.

(Professor of Experimental Physics in the Royal College of Science, Dublin.)

In a paper read before the Royal Dublin Society, Mr. G. Johnstone Stoney has given a new and beautiful explanation of the so-called spheroidal state of liquids, wherein he showed that the force detected by Mr. Crookes, and which is the cause of the motion of radiometers, was also competent to explain the phenomena of the spheroidal state. A liquid drop is said to be in the spheroidal state when falling upon a hot body it does not come into contact with the surface but rolls over it as a flattened spheroid. A mobile elastic spring evidently buoys up the drop until such times as the hot body cools, when, with a sudden rise of temperature and generation of steam, the drop comes into contact with the surface below it, spreads out into a film, and rapidly disappears into vapour.

Hitherto this phenomenon has been regarded as due to the fact that the proximity of the hot surface converts a portion of the liquid into vapour, the elastic force of which sustains the drop. There are, however, several phenomena, applied to the spheroidal condition, to which this generally received explanation gives no solution,—such, for example, as the mobility of light powders in a hot crucible, or the formation of globules on the surface of water and other liquids. Mr. Stoney's explanation, on the other hand, embraces the whole of these outstanding and hitherto enigmatical phenomena. Briefly stated, this theory is based on the fact that whenever two bodies at *different temperatures* are brought sufficiently near each other, a modification takes place in the molecular structure of the layer of gas or vapour between them, given rise to the so-called "Crookes' force," wherein there is an excess of pressure in the direction joining the hot and cold surfaces over the pressure in transverse directions. Now this excess of pressure depends partly on the quantity of heat making its way across the intervening layer of gas or vapour, and partly on the proximity of the two surfaces,—a proximity not to be estimated absolutely, but with reference to the length to which a molecule of the gas will travel in the intervals between its encounters with other molecules. Hence there are obviously three modes whereby the excess of pressure, this Crookes' force, may be developed or augmented:—

1st. By lengthening the paths of the molecules between the warm and cool surfaces, accomplished by attenuating the gas.

2nd. By bringing the hot and cold surfaces very near together.

3rd. By increasing the difference of temperature between the two surfaces.

Now if the support of the spheroidal drop be due to this Crookes' force a difference of temperature must exist between the drop and the surface over which it stands, and the greater this difference of temperature the larger the drop that ought to be supported, and the more persistent the phenomenon. Mr. Moss has shown (Proc. R. D. S., Dec., 1877) that by securing a continual difference of temperature a globule of ether may be supported on the surface of its own liquid for upwards of an hour, until in fact some accidental derangement occurs. The conditions of the two theories being thus defined, it is easy to see that several crucial experiments might be devised which should help to decide the question at issue.

The following experiment the author has made with this object in view. Upon the surface of the ordinary petroleum of commerce, liquid globules of transient duration can readily be formed, simply by removing a small quantity of the liquid in a pipette and carefully depositing a drop on the surface of the liquid. These drops are clearly in the spheroidal condition, and they are easily and abundantly formed by dipping a vibrating tuning-fork into the liquid, or by drawing a fiddle bow over the edge of the vessel containing the liquid. According to the ordinary explanation the drops are supported by the elastic force of the vapour of the liquid, which would, of course, be greater the higher the temperature of both liquid and drops. According to Mr. Stoney's theory the drops are supported by the Crookes' force, generated by the proximity of the drop and liquid, and by the fact that they are at different temperatures. Evaporation rapidly cools the drops jerked up from the liquid, and thus a slight difference of temperature instantly comes into play. If, however, Mr. Stoney's theory be true, then a drop of cool petroleum would be more easily and longer sustained on a surface of warm petroleum, or *vice versâ*, than a drop taken from the mass of liquid below it, where only a slight temperature difference is created.

Two beakers were filled with petroleum from a common source, one (A) at the temperature of the air, the other (B) at a temperature of 100° F. With a pipette some liquid was taken up from A and a drop carefully deposited on its own surface, a globule was formed, floated for a fraction of a second, and then disappeared. The same occurred with a drop from B placed upon B. A drop of B was now removed and deposited on A, a large globule was easily formed on the surface, floated about from 10 to 20 seconds, and then disappeared. A drop of A was now placed on B, the same thing occurred, but the duration of the drop was not quite so great, owing to the greater density of the cool drop tending to sink it below the surface of the warm liquid, thus rapturing the Crooke's layer and destroying the difference of temperature.

There is no doubt or uncertainty whatever about this experiment, and it shows that, if the ordinary explanation be correct, the second case, where B rests on B, should give the best result, whereas the reverse is the case. Further, the experiment wherein the best result is obtained, is such as best fulfils the condition of Mr. Stoney's theory.

The limit of formation of these spheroids, when the liquid is uniformly dropped through a gradually-increasing height, may be employed to test the relative degrees of force which sustain the globule, and careful experiments made by the author in this direction still further corroborated the truth of Mr. Stoney's views.

The Light of the Stars.—For a number of years the special work carried on at the Harvard Observatory, under the direction of Professor Pickering, has been the measurement of the intensity of the light of the heavenly bodies. Some of the results presented at a recent meeting of the Society of Arts, at the Institute of Technology, Boston, indicate measurements almost incredibly fine. The light which falls upon the earth from the satellites of Mars, for example, is about equivalent to what a man's hand on which the sun shone at Washington would reflect to Boston. The labour of measuring the brightness of all the visible stars was begun two years ago. It has since gone on at the rate of about 40,000 a-year, and will be completed next fall.—*Scientific American.*

XUM

AN INSTRUMENT FOR DRAWING THE CONIC SECTIONS.

By Thomas Thorp.

THE instrument shown in the accompanying sketch, Fig. 1, may be used for obtaining figures of the conic sections. It is quite original with me, and can be made without any great difficulty.

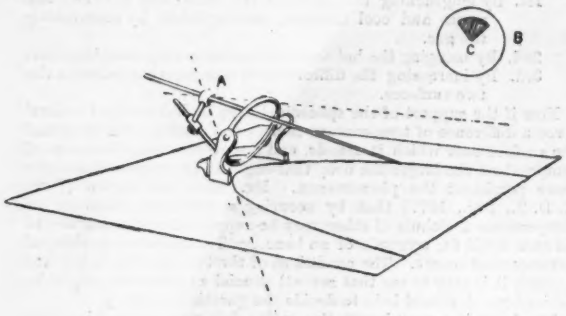


Fig. 1.

The rod through the ball and socket joint at A should be as enlarged at B, the edge in line with the scribing-point being exactly central, as shown at C.

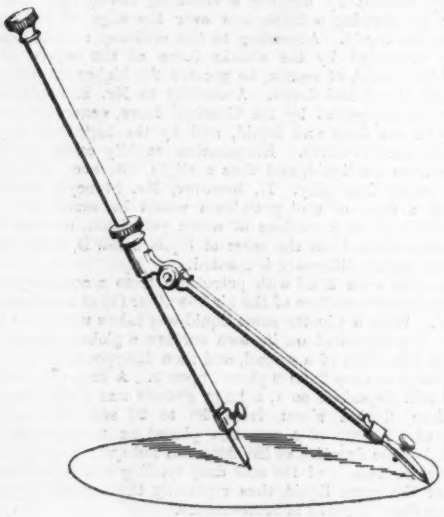


Fig. 2.

The instrument is sketched as describing a parabola, but is equally suited for any other of the conic sections. Further details if required.

For describing ellipses, the instrument shown, Fig. 2, is extremely useful, and may not be known to many of your readers.

Our Next Number.—We have been obliged to defer to next week two illustrated articles, one by a "Fellow of the Royal Astronomical Society," on Work with a Small Telescope; the other by the Editor, on the Great Pyramid. In compliance with the wishes of many readers, the star maps, which have hitherto appeared weekly, will be combined into a single map (on the same scale), which will appear in the first week of each month. In the other weeks some subsidiary maps will be given, illustrating the motions of the heavenly bodies during the month. There will also be given each month a paper dealing with star names, constellations, figures, &c.—[Ed.]

Errata.—The comet referred to in Mr. Denning's letter of last week (letter 122, p. 140), was not the one which bears his name (comet *f*, 1881), but comet *g*, 1881. The letter *g* was misread. In Reply to 55, p. 146, last line but one, for Raper, read Rosser.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of Knowledge, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

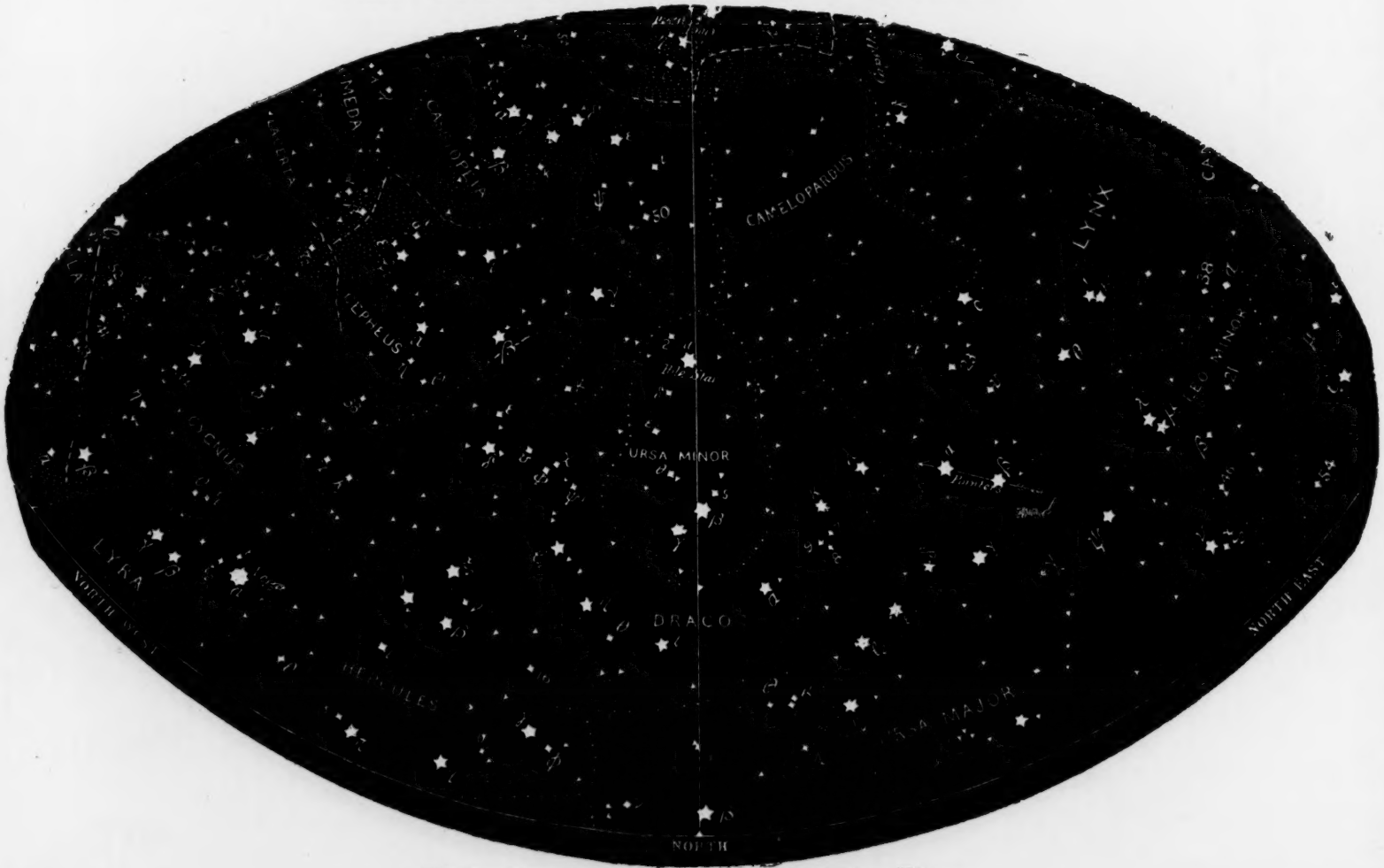
PLANS FOR THE NEW YEAR.—DARWIN AND DESIRE.—SPIRITUALISM.—PLANETS HIDING STARS.—VIBRATIONS OF LIGHT IN ÆTHER.—VECTORS VESTIGES OF CREATION.—NEWSPAPER SCIENCE.—THE GREAT PYRAMID.

[136]—With the first number of the year 1882 we shall somewhat modify our plan, experience and the suggestions of many correspondents having shown that some features which we have not yet introduced would be desirable, and that others which we had thought desirable are either not so, or inconsistent with some presenting greater advantages. The task of selection is not altogether easy. We have been reminded much of the old man with his donkey and two sons in the fable, whose fate, it will be remembered, when he tried to please every one, was to be thrown into the river, along with his three companions. We are urged—(1) to widen our space for correspondence; (2) to keep out correspondence; (3) to encourage paradoxers to disport themselves in our pages; (4) to silence them utterly; (5) to explain simply the principles of every science on the face of the earth, all at the same time; (6) to leave all such explanations to text books; (7) to increase our size; (8) by no means to do so; (9) to publish all the four maps for the month at the beginning of each month; (10) to publish them weekly, as we do, but with a page or two of explanation, &c.; (11) to have the stars punched out of the paper; (12) to have the names of all the stars; (13) to have as little writing on the maps as possible; (14) to leave out the small stars; (15) to letter them as well as the others; (16) to give also weekly zodiacal maps, with paths of planets, moon, comets, &c.; (17) to announce all astronomical phenomena; (18) to remember that the almanacs do that for us; (19) to have more astronomy; (20) to have less astronomy; (21) to be rather less simple; (22) to be more simple; (23) to give reports of societies; (24) to give digests of reports; (25) to give no reports at all; (26) to announce meetings of societies; (27) to devote space to no such purpose; (28) to limit the mathematical column to the discussion of really difficult problems; (29) to use it to explain matters for beginners; (30) to omit it altogether; (31) to extend the whist and chess columns; (32) to shorten them; (33) to leave them out; (34) to have exchange columns, sixpenny sale columns, &c.; (35) to have none of these; (36) to offer prizes, have essay columns for girls and boys, &c.; (37) to eschew such allurements; (38) to have longer articles; (30) to let no article exceed a page, and few be longer than a column; (40) to have articles on ancient philosophy, modern scientific biographies, art and the drama, puzzles, enigmas, hints for collectors of shells, medals, seaweed, &c., &c., &c., *ad infinitum*. To all which, all that we can reply is that we will do our best to please the greatest number.

Mr. Newton Crosland, accusing me of verbal shuffling, claims from me an apology. Darwin, he says, does speak of the "endeavour," though not of the "desire," of the rattlesnake to frighten away its enemies; and the two mean the same. Truly they differ little; but what Mr. Crosland said was that "Darwin attributed the

[*Continued on page* 163.

THE NORTHERN SKIES IN DECEMBER.

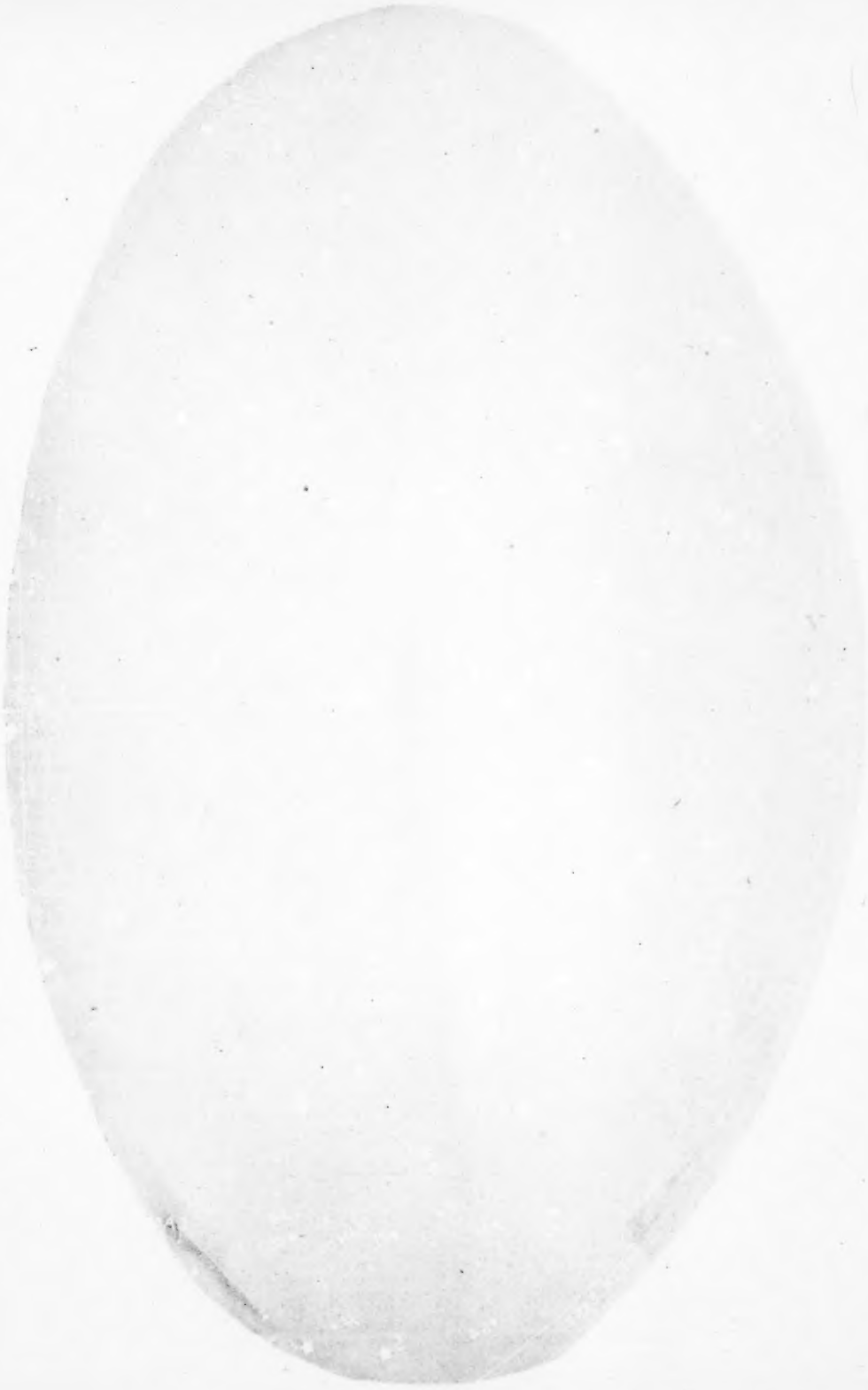


This Map shows the position of the stars in the Northern Skies: —

On November 30, at 10½ o'clock.	On December 8, at 10 o'clock.	On December 16, at 9½ o'clock.	On December 23, at 9 o'clock.	On December 30, at 8½ o'clock.
On December 4, at 10¼ o'clock.	On December 12, at 9¼ o'clock.	On December 19, at 9¼ o'clock.	On December 27, at 8¼ o'clock.	On January 3, at 8¼ o'clock.

The stars in the northern skies are carried from left to right under the pole, and from right to left above the pole, descending to the left or west of the pole, and ascending to the right or east of the pole. They are carried, in fact, round the pole in a direction contrary to that in which the hands of a watch move.

*** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.



Continued from page 160.]

evolution of the rattlesnake to the desire—or endeavour—of the animal to frighten its enemies," and it is just here that he misunderstands Darwin. The theory he attacks is Lamarck's, not Darwin's. Darwin and Lamarck may agree in saying that the rattlesnake uses his rattle with the desire (or in the endeavour) to frighten its enemies; but whereas Lamarck attributed to such desire or endeavour, and the resulting "habit," the development of the rattlesnake, Darwin would do nothing of the sort (rather attributing the habit to the development). If Mr. Crosland will read Darwin's remarks on the views of Lamarck (preface to "Origin of Species," and elsewhere), and remember that Lyell became a convert to the general theory of evolution only when the Darwinian theory replaced Lamarck's, he will see that there is an important difference between the two. Mr. Crosland's diatribe on Dr. Darwin would only be permissible if he had been violently and personally attacked by that eminent geologist, which, I take it, has not happened. May I venture to remark that if Mr. Crosland thought me really guilty of shuffling, he would not "expect to receive an apology from me." He has quite wrongly accused me of shuffling, but I do not want an apology, and certainly I do not expect one.

"Tentative" thinks we do wrong in excluding spiritualism. He thinks it would be "a great gain if we would encourage a searching inquiry into the causes, electric or other, of the singular mind problems continually presented to us." We will do so, by publishing the first scientific results of such inquiries which may reach us. A story he relates seems to illustrate only the effect of an excited imagination.

Ebenezer Kelby writes respecting my remarks about the Great Pyramid, that if certain singular coincidences were noted, and those who had noted them were told it was mere accident, they would say, with a sceptical sneer, "it is a very strange accident," and many readers of KNOWLEDGE say the same of my "opinion with regard to Mr. Baxendell's wonderful calculations showing the wonderful correspondence of pyramid measures with astronomical data." If Ebenezer Kelby will wait awhile, he may find I can give reasons for my remark that the very closeness of some of the coincidences noted by Mr. Baxendell, and of some other coincidences which he has not mentioned, disproves the argument from coincidence.

"O. R. B." asks why occultations of stars by planets, which must be continually occurring, are not alluded to or tabulated? They occur far less often than he imagines. Very few are recorded, and still fewer have been properly observed. I know, indeed, of only one really satisfactory case, viz., the occultation of a sixth magnitude star in Aquarius by Jupiter, well observed by Ellery and Turner with the great Melbourne reflector. The star disappeared gradually, and was visible (just before it disappeared) at a depth of more than 500 miles below the apparent surface of Jupiter—doubtless, therefore, through a great range of the planet's cloud-laden atmosphere. "O. R. B." is mistaken in supposing the size of a star could be determined by noting how long it was in disappearing behind the comparatively slow-moving planet. If a planet's outline were sharply defined, the disappearance even of the largest and nearest star would be to all intents and purposes instantaneous.

"Practical" sends an interesting letter on the use of flesh meat, which shall appear as soon as we can find space for it.

"G. P." asks how it is that if the elasticity of the æther is almost infinite, vibrations set up in it by solar influence have such short duration? Why should darkness so soon follow sunset? The vibrations may be compared to those on the surface of water. When a stone has been thrown into water, the vibrations travel all around, ceasing first at the place where they began, and thence all round as the wave circle expands. The elasticity of the water shows itself in the wide expansion of the wave circle, not in the continuance of the oscillation at any point or points traversed by the wave. So with the æther of space, its elasticity is shown by the great distances to which light travels without appreciable extinction. The æther cannot at the same time carry on the light impulse from each point passed, and continue its vibrations there. Another question by "G. P." has not been answered, that respecting the use of vectors in Maxwell's little book on Matter and Motion. I venture to express the opinion that in a work of that kind, the use of vectors is as entirely out of place as the use of Latin and Greek technical terms would be in a treatise on domestic medicine. To the mathematician, vectors are of use to shorten reasoning and simplify statements, precisely as technical terms are of use in science. But they should have no place in elementary treatises.

We have received many letters besides those published, relating to the "Vestiges of Creation." Pressure on our space, and the comparatively small importance of the subject, prevents our publishing these. It is well known that Robert Chambers was the author of the work, but that he had excellent reasons when he wrote it, and during many years after, for not desiring to acknowledge that it was his. Lyell, in his Antiquity of Man, says of the work, that "written in a clear and attractive style, it made the English public familiar with the leading views of Lamarck in transmutation or progression, but brought no new facts or original line of argument to support those views, or to combat the principal objections which the scientific world entertained against them." Darwin says, "from its powerful and brilliant style, the work, though displaying in its earlier editions little accurate knowledge, and a great want of scientific caution, immediately had a very wide circulation; in my opinion it has done excellent service in this country in calling attention to the subject, in removing prejudice, and in thus preparing the ground for the reception of analogous views." Lieut.-Col. Ross writes us that he has overwhelming evidence to show that Sir C. Lyell *must* have written it; but Lyell opposed the Lamarckian hypothesis; also that Chambers could not have written it, but in the later years of his life Chambers acknowledged to many that he had done so. A.T.C., Eclecticus, and others, have written very fully on this subject. "Eclecticus" regards Darwin's work as a fit and complimentary (query complementary) sequel to the "Vestiges," which reads to me very much as though one should say that Newton's "Principia" was a fit sequel to Kepler's "Prodromus." The question of scientific value, be it understood, is not as between the work of Darwin and Chambers, but as between the theory of Lamarck and that of Natural Selection.

It is pointed out by a writer, who desires that his name may not be published, that a newspaper paragraph recently commented upon in our columns somewhat unfavourably, correctly represented the statements of a French chemist in the *Comptes Rendus*, and that newspaper science does not deserve all the vilification it receives. As I have myself written a good deal about science in the newspapers, I need hardly say that I am not disposed to regard *all* newspaper science as unsound. Still, it remains true that newspaper science cannot be regarded as trustworthy, simply because the general reader cannot distinguish the sound from the unsound, and has no means of ascertaining to whom particular statements are due, while it is well known that some editors of leading daily papers have themselves no knowledge whatever of science, and would as soon insert a column of utter nonsense, if foisted on them as the work of a known science student, as the most carefully-reasoned article by a Darwin, a Tyndall, an Airy, or a Huxley. From my own experience I know that many newspaper editors have no idea whatever of the progressive nature of science. Articles which I have written, the appearance of which has been delayed for one reason or another, would have been inserted just as they were written, had I not insisted on the proofs being sent to me for changes rendered necessary by the lapse of time. In one case, an article which I wrote for the *Times* immediately after the eclipse of June, 1878, was recast by me in this way several times, at great cost of labour, until at last, when I left England for America in the autumn of 1879, I gave up further attempts to make the article fit for reading at the time of publication. I had supposed my labour lost, but, to my horror, I saw that very paper quoted in an American newspaper in December, 1879, and thus first learned that at last it had appeared in the *Times*. Other papers were delayed with the intention of eventually using them, until, at last, I gave up all idea of their being patched into fitness for later dates. Some articles I have seen in the daily papers suggest to me, by their time-worn aspect and other evidence of decrepitude, that all writers are not honest enough to remind the editor of the deterioration a scientific article is undergoing as time passes (or has already undergone), preferring that an article should appear long after it has ceased to have any value than that they should lose the money which is only paid after an article has appeared. In America, the editors of leading papers adopt a plan which is at once more sensible and—in my judgment—more honest, making the question of payment (so my esteemed friend, the editor of the *New York Tribune*, tells me) independent of the use of an article which has been accepted and sent in type to an author for correction. This surely is better than the system of some of our English leading papers (the *Times* amongst others), by which an unskilful editor, after inviting and accepting more contributions than he can find room for, calmly suffers the loss to fall on the authors, and selectively on those who may be honest enough to tell him that their papers no longer have the value they originally possessed. It is because all writers are not careful to do this that we so often see old truths presented as novelties, and exploded errors presented as accepted truths, in our daily papers.

Mr. Baxendell has written a letter, in reply to Mr. Ranyard's remarks, letter 85, p. 113. We would insert the letter, though it does little more than express Mr. Baxendell's unchanged confidence in his views, were it not for passages showing that Mr. Baxendell has entirely misapprehended Mr. Ranyard's remarks respecting fortune-telling, weather prediction, alchemical promises, and

XUM

forth. Mr. Ranyard, we are sure, had no thought of citing Mr. Baxendell as an example of the objectionable class of science workers (or rather of persons who want to work science) to whom he referred. Mr. Ranyard's letter has been regarded by quite a large number of correspondents as unduly considerate towards paradoxers, while others consider it as very gentle towards them, but not unwarrantably so. Richard A. Proctor.

SIR RICHARD PHILLIPS.

[137]—"Delta" offers to send weekly paragraphs, giving startling and important theories propounded by Sir Richard Phillips, which he believes would be read and studied with profound interest by readers of Knowledge. After reading the specimens "Delta" forwards, I decide that only one paragraph relating to Sir R. Phillips shall appear in these columns so long as they are under my charge. It is not one of his own writing, but by the late Professor De Morgan, and runs thus:—

"Sir Richard Phillips had four valuable qualities—honesty, zeal, ability, and courage. He applied them all to teaching matters about which he knew nothing, and gained himself an uncomfortable life and a ridiculous memory." Richard A. Proctor.

RETROGRADATION OF A PLANET. — ORRERY. — SOLAR HEAT. — ASTRONOMICAL SLIDES. — DENSITY OF NEPTUNE. — VULCAN. — THE GRAVITY OF SIR R. PHILLIPS. — PLANETARY RINGS. — THE GREAT BEAR. — SIRIUS AND ORION. — ALGOL AND MIRA. — STAR LETTERS AND NUMBERS. — NEW STAR IN CASSIOPEIA. — VENUS IN SUNSHINE. — VELOCITY OF LIGHT. — EVOLUTION. — RAINBOW. — SOLAR STORMS. — ANTIQUITY OF THE PYRAMIDS. — MENTAL PHYSIOLOGY. — THE ANTIQUITY OF MAN. — TOADS.

[138]—As no one, so far, appears to have replied to the Query (17) of "Astronomicus," on page 60, perhaps I may say, shortly, that the seeming retrogradation of a planet is a differential phenomenon, arising from the fact that the earth moves less rapidly in her orbit and describes a larger ellipse than the interior planets Mercury and Venus; and more rapidly than those exterior to her—Mars, Jupiter, Saturn, Uranus, and Neptune, whose orbits must obviously increase in size as they recede from the sun. If "Astronomicus" will describe a series of concentric circles to represent the paths of the planets, and suppose them to travel round such circles in a direction opposite to that of the hands of a watch, and at rates proportioned to their periodic times (to be obtained from any Primer of Astronomy whatever), he will soon see how, taking the case of Mars, for example, when the earth is between that planet and the sun, she, as it were, overtakes him. Evidently in doing so Mars will appear to go back, as it were, among the infinitely distant stars; as will, in a similar manner, the planets outside him when in and near opposition. So far the appearance of retrogradation may be called subjective. I may now add, what I abstained from saying in connection with Mercury and Venus, for fear of confusion, that they have also an objective movement of retrogradation, when they come between the sun and the earth, as your correspondent may see from his own figures. Mars is the planet which describes the largest retrograde arc, the reason for which will also be apparent from his diagram.

"Vulcan" (query 20, p. 80) may see an orrery at South Kensington.

If "Warmus" (query 30, p. 101) possesses a pair of compasses, he may clear up part of his difficulty by constructing a simple figure; or he may even do so by the aid of a bronze halfpenny. Let him, then, describe a circle one inch in diameter, and from the top and bottom of this draw two parallel straight lines, between which he may rule as many more as he likes strictly parallel to them. These he may take to represent the sun's rays. Now, let him get a strip of card one-inch long, and hold it square across his series of lines. Obviously they will all fall upon it. If, however, he holds it slantingly across them, he will see that fewer and fewer of them touch it as its obliquity increases. Now this is just what happens on the earth. Within the tropics, where the sun is always somewhere overhead, all his rays fall square to the observer's horizon. Where, however, as in high northern and southern latitudes, the sun never, even at noon, reaches any great elevation above the horizon, his rays necessarily fall obliquely, and we receive comparatively few of them. With reference to the concluding part of "Warmus's" question, the gentleman who gets tanned by the sun in a tropical country has his complexion spoilt by the direct and immediate action of the sun on the *rete mucosum*, or under layer of the skin. It is this that sunlight enters directly, and not the earth first!

If "C. J. S." (query 33, p. 101) only wants his slides for one night, he will find that patent plate glass, densely smoked by burning camphor, will give him an absolutely opaque ground, on which very effective diagrams may be made by the aid of etching and sewing needles and compasses with a needle point. He must gum a little bit of paper on before smoking his slide, as a centre for one leg of the compasses to rest on, as otherwise they slide about the glass and make undesirable lines and scratches.

Assuming that we are looking at the outline of the *solid body* of Neptune, then I may tell "S. S. S. S." (query 38, p. 101) that his density must be something like 0·96; that of water being 1. Only one satellite is known. Its period is 5 d. 21 h. 3 m.

In answer to "S. S." (query 41, p. 101), there is a practical concensus of opinion among astronomers that Vulcan has no existence whatever. I am not denying that there may be a planet, or planets, between Mercury and the Sun, but the pseudo-observation of Lescarbault was sufficiently definite to enable computers to calculate the orbit of the alleged body. This having been done, it *must* have reappeared on the sun's disc on several occasions since, when the most skilled observers, employing powerful instruments, have wholly failed to see anything. It is the "Mrs. Harris" of the solar system.

"Meter" (query 45, p. 101) appears to be unaware that scientific men pay about the same kind and amount of attention to the lucubrations of Sir Richard Phillips that they do to those of, say, "Parallax," or Mr. John Hampden.

The same correspondent (succeeding query) should buy and read "Saturn and its System," by the Editor of this journal.

Mr. St. Clair (query 57, p. 101) will find that the principal stars in the Great Bear all set in latitudes below 25° North. The second part of his query is, in one sense, meaningless; because in the case of a circum-polar constellation, what is east below the Pole, becomes west above it, and *vice-versâ*; but, in the sense that the right ascension of Benetnasch exceeds that of Dubhe by some 2 hours 47 minutes, of course the latter star is the westernmost, as by "longitude" your correspondent obviously means right ascension. I may say that Ursa Major straggles over six hours in the sky, as nearly as may be from o on one side to Σ 1830 on the other. I quite fail to understand the meaning of the constellation having "a cycle of 2,700 years;" nor am I in any better position to clear up the difficulty as to the supposed connection between Sirius and either Ursa Major or Minor.

In answer to query 58, p. 102, Sirius was one of Orion's hounds, but that it was ever placed on the "shoulder" of the giant is news to me.

The great comet spoken of by "S. C. H." (query 61, p. 102), has no other designation than comet *b*, 1881. Its orbit was seemingly parabolic, and the parabola—as "S. C. H." doubtless knows—is a curve which does not return into itself.

"S. C. H." (query 62, same page) will find Algol marked on the map on p. 97. It is in the head of Medusa.

"F. H. S." (query 63, p. 102) asks when Algol and Mira are at their brightest? In the case of the first-named star, he should rather ask of the date of its minimum, inasmuch as it shines as a star of the second magnitude for about 2d. 13h., diminishes to the fourth in three or four hours, remains as a fourth magnitude for about twenty minutes, and again increases in brightness until it regains the second magnitude. It passes through the entire cycle of its changes in 2d. 20h. 48m. 55s. For its next minimum, "F. H. S." must really watch for himself. Mira Ceti should attain its next maximum about June 16 or 17, 1882.

Stars, I may say, in answer to query 64 (p. 102), are both lettered and numbered in their order of right ascension.

By the periodical star, concerning which "F. H. S." puts his next query (65), his informant probably meant to imply that if the one which blazed out somewhere about RA 0h. 19m., and Dec. 63° 24′ N. in November, 1572, was identical with those which appeared in or near the same spot in 945 and 1264, it may possibly soon reappear now.

Next, I would tell him (query 66) that Venus is always visible to the naked eye in bright sunshine to anyone who knows where to look for her—at and about the time when the *Nautical Almanac* gives the intimation "♀ at greatest brilliancy."

It may be worth while to remark, in connection with letter 100 (p. 116), that the latest and best determination of the velocity of light, gives it as 186,380 miles a second. Hence, as the pace of the fastest express never exceeds sixty miles an hour, it must travel $\frac{1}{60}$th of a mile in a second, so that the velocity of light must be in reality about 1,118,000 times that of the train.

Mr. Mitchell (letter 107, p. 117) might advantageously study Professor Huxley's lecture on the convincing evidence, from fossil remains, of the direct descent of the horse from the Hipparion, delivered before the Zoological Society in December, 1880.

"G. S. M." (query 67, p. 122) should read Tyndall's "Six Lectures

on Light," published by Longmans, for an explanation of the rainbow. It is too long to give here. Unless the same correspondent (query 68) knows something of the theory and action of the spectroscope, I am afraid that he will not be much wiser when I tell him that the velocity of solar storms is measured by the displacement of certain lines in the spectrum of the sun's surroundings.

I am not familiar with Rawlinson's researches in Egyptian history, but "Actinolite" (query 72, p. 122) will find an exhaustive argument in Bunsen's "Egypt's Place in History," tending to fix the date 3620 B.C. as that of the Pyramids.

"S. S. S. S." (query 79, p. 122) cannot possibly do better than get Carpenter's most interesting and amusing "Mental Physiology."

The researches of Mr. Horner, concerning which "Clio" puts query 84 (p. 123), were undertaken between the years 1851 and 1854, partly at the cost of the Royal Society, but mainly at that of the Viceroy, Abbas Pacha. No less than ninety-five shafts were bored through the alluvium of the Nile Valley, through sediment which the French engineers calculate is formed at an average rate of 5 inches in a century. At depths varying from 16 to 24 feet, "jars, vases, pots, and a small human figure in burned clay, a copper knife, and other entire articles were dug up." "Pieces of burnt brick and pottery, Sir Charles Lyell moreover tells us, were extracted almost everywhere, and from all depths, even where they sank 60 feet below the surface." If then we divide 60 feet, *i.e.*, 720 inches, by 5, we get 144, and as the estimation of the French engineers appears entirely trustworthy, that 5 inches of Nile alluvium are deposited in a century, this shows that pottery and brickmaking were practised by the Egyptians at least 14,400 years ago.

In connection with query 87 (p. 123), I should like to put one myself. *Do* toads ever exist "for many years, enclosed in blocks of solid matter?" I fear that "Arachnida" is the victim of the "Great Gooseberry" column of some local newspaper.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

"THE COMMON STAR-FISH, CROSS-FISH, OR FIVE-FINGERS (*URASTER RUBENS*, LIN., AG.), OCCASIONALLY THE FOOD OF THE SUN-STAR (*SOLASTER PAPPOSA*, LIN., FORBES)."

[139]—Sir John G. Dalyell, in his excellent work, "The Powers of the Creator displayed in the Creation" (Vol. i., p. 3), in speaking of the common sun-star, states that "the fishermen believe that this animal devours the *Asterias glacialis*" (*Uraster glacialis*, Ag., Lin., Spiny Cross-fish). I am able to confirm this assertion, so far, at least, as it relates to the closely-allied, but far more common species, the *Uras'er rubens*, or common five-fingers. Whilst on a visit for some weeks at Colwyn Bay, North Wales, during the months of September, October, and November last, I had ample opportunities of observing some of the habits of the sun-star, great numbers of which I found amongst the sea-tangle (*Laminaria*) beds, which, forest-like, waived their fronds in the tidal waters close to the fishery weir at Rhos, in the possession of Mr. Parry Evans. At the time of the low equinoctial tides, this locality abounded in specimens of *Solaster papposa*; it was the commonest species present. I could have collected wheel-barrows full. Of course, the common star-fish, or five-fingers, was there also—it has "a finger in every pie"; but it did not abound to the same extent as the sun-star. Here and there I met with *Cribellæ*, both *Oculata* and *Rosea*, as well as other members the Asteroidean and Ophiuridean order of the Echinodermata. It was a very common thing to notice within the stomachs of the larger individuals of *Solaster*, rays of the five-fingers; sometimes there were two, but more generally one ray in a stomach. Now the ray was fresh, having been recently swallowed, and unaffected by any digestive process, now in a state more or less pulpy from the results of that process.

It has been long known that the food of star-fish consists, in a great measure, of the succulent portions of different kinds of molluscs, such as mussels, oysters, scallops, &c., small crustacea, and other animals, which they kill and devour; but so voracious are they, that they will sometimes make a meal of one of their own kind. It is difficult to keep sun-stars alive for any length of time in an artificial state; they soon die, so that I was unable to discover by experiment in what form of diet their propensities might be supposed to lie. Neither was I able to ascertain whether the sun-star attacks the whole living five-fingers, by first of all seizing the pointed part of the ray and gradually sucking in its whole portion, thus causing it to break off from the disc, or whether the remains found in the stomach consisted of rays which had been, either voluntarily or otherwise, detached from the body. The destruction which the five-fingers cause to mussel and oyster-beds is well known, and statements to this effect are corroborated by ample testimony. Fishermen and others practically interested in oyster fisheries, when examined before appointed Commissioners on this subject, bear testimony, one and all, to the havoc occasioned by these star-fish. Captain G. Austin, some time ago engaged in the oyster business at Whitstable, gave evidence before the Commissioners in the following words:—"They (the five-fingers) come like a flock of gulls, and, unless the beds were well dredged, they would soon destroy the spat. There is one kind that will eat an oyster itself, yet it is a singular thing with regard to them that after they have been dredged for a time, they roll themselves up and float away. So much is that the case, that in places where the fishermen have caught ten bushels of five-fingers one day, they will go out the next day and not catch one." (See Report of the Commissioners appointed to Inquire into the Sea Fisheries of the United Kingdom. Vol. II., 1865, p. 1,363.) There is every reason to believe that the sun-star is also destructive to oysters; if these asteroids are found, which is often the case, on oyster beds, it is presumable that they are there for some purpose, and when we know that other closely-allied members of this order possess in an eminent degree, and when opportunity offers gratify, oyster-eating proclivities, it is almost certain that their presence on the oyster or mussel beds has a gastronomical explanation. Sir J. G. Dalyell has recorded instances of sun-stars exhibiting cannibalism, and has been an eye-witness of larger specimens devouring their smaller brothers. The destructive agency of the sun-stars with respect to oysters must be small compared with that wrought by the ubiquitous five-fingers, for though the former are widely distributed round our coasts, they are not so generally abundant as the latter. That the sun-star frequently feeds upon the five-fingers I have lately had proof, but whether the good they do in this respect is outbalanced by the evil they cause to oysters, or whether the good has any appreciable effect on the natural increase of the five-fingers—these are questions I cannot answer.—Yours, W. HOUGHTON, M.A., F.L.S.

WORD-CHOICE.

[140]—I hold that an Editor who amply lends his pages for the actual use of the public at large, as well as for delight and teaching, has a right to look for every aid that public can give him. Each writer offering matter for insertion should be asked to consider the *length* of every word he sets down. While thus sparing the printer's costly time, and leaving blank the space otherwise needlessly filled, he will at the same time be serving his own ends, since more space, more letters. Not only so, but in another way he will be a gainer. He will soon find that he is getting to write better Saxon English. Rather write *enough* than sufficient, for instance; *begin* than commence; as well as letting us have *using* instead of employing when the sense permits. You can *use* your time, though you must *employ* a workman. In the above few lines I had myself tripped. I was about to write "entertainment and instruction." Besides being shorter, "delight and teaching," are, I think, better.

WORD-LOVER.

ARE WOMEN INFERIOR TO MEN?

[141]—I have just seen KNOWLEDGE for December 2, and have read with much regret a letter (numbered 61), signed "Susan G.," relating to your interesting article "Are women inferior to men?" If the writer is to be considered a representative of her sex, I fear she has injured the cause she wished to advance. In the first place, some of her statements are not accurate. She says "in a girl's education the brain is but slightly exercised." Now in point of fact, in the present day, girls' education is quite as comprehensive as boys', with less relaxation in the way of cricket, football, &c., to counterbalance the study. "Music and needlework," your correspondent thinks, "scarcely exercise it (the brain) at all." I wonder what Haydn, Handel, and Bach would say on that point? If music *is* music, and not mere mechanical "strumming," it cannot be called a brainless study. And yet it has to be worked at in the hours that boys devote to football.

As to "Susan G.'s" theory that whipping would develope brain, it appears to me that people with brain do not want whipping. The two great incentives to study are "interest in the work" and "love of the master," and if those who have the training of boys and girls can inspire these, as every good teacher can, the brain results will be much better than those shown by a girl of eighteen taken from her natural pursuit (?)—"husband-catching," and whipped into a course of study for which she has no inclination.

Having entered a protest against the whipping theory, may I ask you, for my own information and that of others, whether a comparison, to be quite fair, should not be between those whose surroundings are similar? Do you not think that as the mind of a man having the care of motherless children or of a sick wife, becomes insensibly domesticated, and almost womanly (as distinct from effeminate), so the mind of a woman called upon to enter a profession or manage a business becomes widened and strengthened by

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contact with the outer world, for the work before it? But whilst the lot of most women (without being doomed to "suckle fools and chronicle small-beer") is yet to fill, as intelligently and happily as may be, the domestic posts of daughter, sister, wife, and mother, would it not be unfair to call upon them to exhibit qualities for which they would have no exercise, and which, if ever needed, come with the need? The "Jack Sprat" story of our nursery days would be falsified if men and women were made exactly alike, and I fear the result would sometimes be that the domestic platter would not be "licked clean." I may be wrong, being

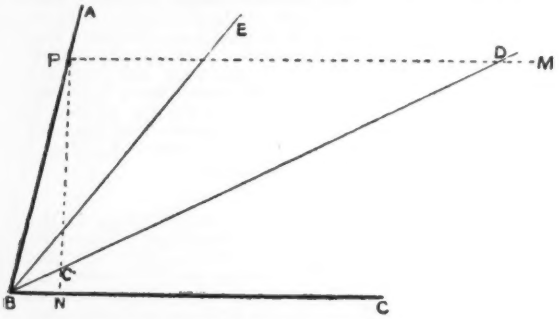
Only a Woman.

P.S. Let me add one word of grateful thanks for your paper. May it have the success it deserves.

Edinburgh, *Dec.* 9, 1881.

ANOTHER TRISECTION OF AN ANGLE.

[142]—I send you another method of trisecting an angle, more suitable for practical purposes. Of course it requires the ordinary mechanical contrivance, but as the point *P* is arbitrary, and *CD* is double *BP*, these measures can be permanently marked on the scale. To trisect any angle:—*ABC* is the angle. Take any point



P in *AB*, draw *PM* parallel and *PN* perpendicular to *BC*; from *B* draw *BD* so that *CD*=2*BP*; bisect *PBD*. The lines *BD* and *BE* trisect the angle. The proof I leave to the ingenuity of your readers. It is very simple.—Yours, &c., S. B.

15, Bessborough-street, St. George's-sq., S.W.

[This is the method invented by Pappus, of Alexandria. From it we can easily see that the hyperbola is a trisectrix.—Ed.]

VARIABILITY OF PENDULUM.

[143]—There is a misapprehension here founded on want of precision in ideas. The writer points out "a difference of velocity," but omits to specify to what point he refers as the standard. Apparently, however, the centre of the sun is intended, for which his statement is approximately correct; but the motions of the pendulum are controlled by the mass of the earth, generally reckoned as a force acting from its centre, and with reference to this latter point, no such inequality of velocity exists; so there is no ground for anticipation of any diurnal variation dependent on this (supposed) cause. Cogito.

ANTIQUITY OF THE PYRAMIDS.

[144]—It is impossible in the space which could be allowed in Knowledge to give adequate reasons for believing some of the Pyramids to be 6,000 years old. If "Actinolite" (query 72, p. 122) wishes to know the best authorities on the subject, he will find Brugsch's "History from the Monuments," Rawlinson's or Erasmus Wilson's "Egypt," and a work published by Macmillan, entitled "A Ride in Egypt," by Mr. Loftie, give him ample information.

If Egyptologists are correct in assigning the Pyramid of steps at Sakkara (which, by-the-way, differs from all others in *not* following the cardinal points of the compass) to Vanephes of the first dynasty, Sir J. Lubbock would probably be within the mark. The Pyramid of Senerferoo, eighth king of the third dynasty, is at Maydoum, and may well be as old as 6,000 years. For the date of Menes is fixed as 5702 B.C. by Bœeckh, 5,000 B.C. by Prof. Owen, Marriette, and Manetho, 4,455 by Brugsch, for which he gives excellent reasons, 4,500 B.C. by Maspero, 4,000 B.C. by Chabas, and 3,892 B.C. by Lepsius. English authorities prefer a shorter chronology, Mr. Stuart Poole even favouring 2,717 B.C.

It may be said that all the accumulating discoveries go to prove, with slight exceptions, the validity of Manetho's list and dates. For instance, one of his supposed mythical assertions with regard to a King Apappus, that he was a giant, and reigned one hundred years, has recently been confirmed by a monument now at Boulaq, mentioning the one hundredth year of his reign, and his hieroglyphic name Papi means a giant. This longevity is surpassed by that of Assa and his son Ptah-hotep of the fifteenth dynasty, who, according to the Prisse papyrus, must have lived over 130 and 110 years respectively. See "Smith's Bible Dictionary," I., 322, where these facts are used as arguments in favour of the long lives of some of the patriarchs.

It is important to recollect that if the monuments are held to confirm the old supposed chronology of about 4,000 years from the creation to the time of Christ, they witness in favour of the plurality of the human race, because they show quite as great a divergence between the negro, Egyptian, and Asiatic types on the earliest monuments as at the prefent time, and so there could not possibly have been a sufficient period for such a differentiation from the progeny of a single pair.

A Member of the Society of Biblical Archæology.

SOME SUGGESTIONS RESPECTING "KNOWLEDGE."

[145]—1. I think it would be a great advantage to many of your readers if in your notices of books (which I hope to see soon) you stated the price as well as the publisher's name and address.

2. *Sale Column.*—Do not you think that a sale and exchange column (such as in the *English Mechanic*) would be a good addition to Knowledge? Through it subscribers could exchange and sell scientific works, telescopes, aquaria, &c.

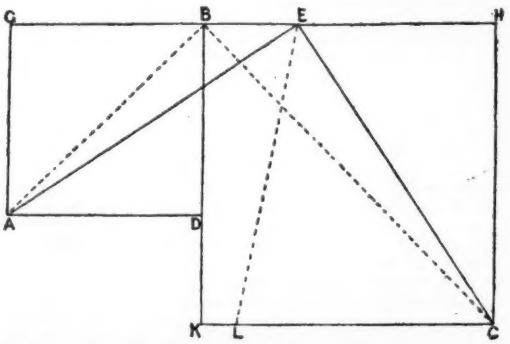
3. *Collections.*—Shall you have papers on making collections of all kinds of natural objects, such as a herbarium, butterfly collections, geological, bird and fish stuffing, skeletonising, insect taxidermy, &c.?—[Freshwater aquaria.—Ed.]

4. I hope you will foretell all natural phenomena, such as eclipses, meteoric showers, &c. (as far as you are able), and how best to be seen, by naked eye or telescope. E. R. C.

THE THREE-SQUARE PUZZLE.

[146]—Mr. Langley's note (No. 97) reminds me of an old puzzle which was set to me in Germany a good deal more than forty years ago. It is, in fact, a variant of his, and shows how his five pieces are obtained. It also always seemed to me to be the best proof of the celebrated 47th proposition of Euclid's first book.

Take any oblong piece of paper and fold down two corners, as along AB, BC in the figure; then, by cutting along AD and CK, you obtain two adjacent squares, AGBD and KBHC. Now, the puzzle is by one cut with a pair of scissors to cut these two squares into three pieces which will themselves form a square. Let those who are fond of puzzles attempt it without reading what follows.



Solution.—Fold the paper so that the point C lies on the point A. Then the fold will be along the line EF, giving the point E, where, of course, EH=GB. Cut with the scissors through both of the folded sides from A to E, and C to E, by one cut. Then you have the two triangles AGE, CHE, and the shapeless remainder. This done, I may leave any child to fit them into a square, and any young geometer to prove that that square must be that on the hypotenuse of a right-angled triangle of which the sides are also sides of the two squares. Observe, that if you also cut along BD, you get Mr. Stanley's five pieces.—Yours, &c., Alexander J. Ellis.

THE ORIGIN OF BUTTERCUPS.

[147]—I am sure that most of our readers could not fail but notice the clear and highly descriptive article upon "The Origin of Buttercups," a valuable treatise upon a botanical subject, without botanical technicalities, and rendered in such a manner that even the least unscientific person reading the article would have his knowledge considerably extended respecting buttercups.

I cannot, however, understand Mr. Grant Allen's theory of evolution respecting cut leaves. He says, "As a rule, just in proportion as vegetation is thick and matted, do the plants of which it is composed tend to develope minutely divided and attenuated foliage." I hold that the rule is the very reverse, for in traversing any waste piece of land where the struggle for existence is left to itself, one sees the very plants that predominate are those with entire leaves, viz., grasses, daisies, primroses, docks, &c., &c., and hold their own against the finely-cut leaved species; indeed, one scarcely sees the buttercup with its cut leaves growing in any profusion on uncultivated ground.

It is only in protected meadows that it flourishes in abundance; whereas the farmer carefully uproots those having entire leaves, knowing well that under their broad leaves no grass will grow.

Those plants having cut leaves are, as a rule, "tall growers," and have a weak and slender stem compared with the height of the plant. Have not these developed cut leaves in order that the wind may not break them down, but allow it to pass freely through? If, on the contrary, they had to develope entire leaves, they would be at a disadvantage in even an ordinary breeze. Upon the same principle many tall plants have entire leaves, but as they approach the top of the stem, the leaves are invariably smaller. Consequently, where the plant is weakest the leaves are smallest.

Again, many plants having entire leaves throw up a flowering skin entirely devoid of leaves, probably learning by experience that if they had to produce leaves there, snap would go their stems —viz., plantain, foxglove, hyacinth, grasses, &c.

It may be argued that few of our large forest trees have cut leaves, and that, therefore, my theory respecting the wind would fail. But I hold that they possess advantages equal to cut leaves in having their blades placed at the end of petioles, and articulated to the stem, so that they may bend and allow the wind to pass without injuring the tree. I hold that petioled leaves are merely a modification of cut leaves. Few of our radical leaved plants possess petioles to their leaves, not being required for such a "lowly" position. West Riding.

BEES AS FLOWER FERTILISERS.

[148]—In the suggestive article by Mr. Grant Allen in Knowledge No. 4, on the "Origin of Buttercups," an error has crept in with regard to bees. I am a bee-keeper on a somewhat large scale, and, consequently, interested in knowing from what flowers bees make their collections; but, though we have abundance of buttercups here, the hive bees do not visit them. Certainly buttercups are not included by bee-keepers among honey plants.

I do not, however, take exception to the assertions of scientific men, that bees are important aids in the cross fertilisation of flowers. When I say that the number of good honey-producing plants are comparatively few, and bees select the best yielders for the time being, I mean the probabilities appear to me to be very much against flowers being dependent upon bees for the fertility of their seeds.

Another circumstance is worth notice. When hive bees make collections of pollen from osiers, they fly straight to and from the tree, without passing from the male to the female flowers, a fact which may be easily verified in almost any district where bees are kept. It appears to me that bees might be of service as an aid in cross-fertilisation in this case; but it is fair to add that pollen is much needed in early spring when osiers bloom. This shows that the presence of honey in flowers is not the only attraction for the industrious bee.—Yours, &c., Alfred Donbavand.

Picton, Chester, *Dec.* 16, 1881.

Professor Nordenskjold is going to Russia, where the Government wishes to consult him on the best method of opening North Siberia to European navigation.

At the late Archæological Congress, held at Tiflis, Professor Samokvasoff gave an account of his discoveries in the graves near Pyatigorst. He excavated about 200 places of interment belonging to the stone, bronze, and iron periods. In the larger graves, bronze implements were found with stone ones. As there were in these graves, besides the bones of sheep, several split human bones not belonging to skeletons, he inferred that during the bronze-period the people in that part of the Caucasus were man-eaters.—*Frank Leslie's Magazine.*

Queries.

[104]—Moons of Mars and Saturn.—1. Can you inform me if Mars has any satellites? If so, how many? What is their period of revolution, and at what distance are they from the planet?—[Mars has two moons, discovered in August, 1877; periods of revolution, 30h. 14m., and 7h. 38m.; distances from the planet's centre, about 6,000 and 15,000 miles.—Ed.] 2. What are the names of Saturn's two outmost satellites?—[Hyperion and Japetus (the latter the outermost of all).—Ed.]

[105].—Fungi and Lichens.—How are specimens preserved, and which are the best books on the subject?—J. S.

[106]—Pneumatic Bell.—Will M. Tester (Letter 79, p. 100) kindly favour me with instructions, "plainly-worded, exactly-described," for making and fixing (down staircases, &c.) a pneumatic bell; taking for granted—as is really the case—that I have very little native ingenuity? Also stating the price of materials used. If he could possibly supply a simple diagram, where necessary, I should be greatly obliged.—E. R. C.

[107]—Name of Star.—How would you pronounce "Betelgeux," one of the stars (*a*) in the constellation of Orion. [The question is one for Arabic students. The name Betelgeux is, we believe, a corruption from *Ibt al Jauza*, the giant's shoulder.—Ed.]

[108]—After-Images.—These seem to be best obtained, not by closing the eyes and keeping them closed, but in the *act* of vigorous winking. When I do this after looking at the sun, I soon obtain an after-image of the following nature: An interior green disc; round that a narrow circle of red, and beyond, an undefined space of violet or dark blue. Has this been noted and sufficiently explained, and, if so, what is the explanation? Is there any significance in the fact that those three colours are the fundamental colours?—Max.

[109]—Railway Collisions.—From the history of these, are any practical rules deducible for the case of imminent collision (rules as to position and attitude in the carriage, &c.), the observance of which might somewhat limit the power of railway companies to maim and murder us?—Max.

[110]—The Coal Age and the Earth's Internal Heat.—As an ignoramus, may I ask you whether the earth's own internal heat had anything to do with the gigantic flora of the coal age, on the principle of a hothouse? Is it supposed that the earth was at one time in a state of intense heat, and will gradually cool like the moon has done? Does not the temperature increase the deeper you sink into the earth? What is a good text-book on this subject? —Ignoramus.

[111]—Axes of the Planets.—Will you inform me if the axes of the planets are all inclined the same way—that is to say, if the sun and all the planets could be placed in a line at their respective distances, would all the axes be inclined in the same general direction? —Orion.—[The axes of the planets have no community of direction. Our earth's axis points towards Ursa Minor, Jupiter's to a point in the constellation Draco, not far from Omega, Saturn's to a point near the northern extremity of Cepheus, and so on.—Ed.]

[112]—Optical Illusion.—A remarkable optical illusion is caused by holding the hand, with the fingers close together, near a gas flame. One seems to see through his fingers. What causes the illusion? Does the light shine through the skin at all?—G. P.

[113]—Illusory Figures of Transparent Solids.—How is it that in looking at the figures of transparent solids that it is possible for you to imagine the figure in two positions? And what adjustment takes place in the eye whilst so doing? For instance, in the case of the cube it is possible for you to see it as having a certain face as the front one, and without moving the eyes you can make the opposite face appear as the front one; or in the prism, the edge xy can be made to appear either at the back or front, just as you please; but no motion of the eyes need take place. Yet there is a slight effort felt as you change, so to speak, the position of the cube or prism.—P. B. S.—[Mr. Foster will discuss such illusions.—Ed.]

[114]—Marine Boiler.—What is the increasing temperature on the furnace crown of a marine boiler required to keep the steam at a pressure of 100 lb. per square inch; with the scale formed by the deposit of salt within the boiler increasing by sixteenths, from $\frac{1}{16}$ to $\frac{1}{2}$ in. thick? How can this be calculated?—Crankshaft.

[115]—Screw Propeller.—Is there a vacuum on the following side of a screw propeller blade when revolving? If so, does it materially affect the power of the engine? Any information on either of the above will greatly oblige.—Crankshaft.

[116]—John Bull.—Can any reader refer me to the origin and meaning of the term "John Bull."—G.

[117]—Parallelopipedon or Parallelepipedon.—Why do

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English writers spell "Parallelopidedon?" Both in German and French this word is always spelt "Parallelepidedon" and *parallèlépipède* (comp. Dictionnaire de l'Académie and Littré's Dictionnaire), and one of the greatest authorities in classical philology assures me that the *o* of "parallelos," being an un-essential termination, *must* yield to the initial *e* of "epi."—G. L.

[118]—How to Make Cotton Waterproof.—Will any of the readers of Knowledge be good enough to indicate a process by means of which raw cotton wool, just as the plant furnishes it, can be made impervious to water, without changing the fibre or colour of the cotton?—Cottonian.

[119]—Electro Plating.—By mistake, nearly six ounces of a cyanide of copper solution have been thrown in two gallons nearly of a double cyanide of silver solution. Would any of your readers be kind enough to tell me how to purify my silver solution?—W. Van Eys.

[120]—Transit of Venus, Dec., 1882.—As the above transit will take place in December next year, I should very much like to know how the calculations are made in order to ascertain the sun's distance from the earth. I have a slight knowledge of how the work is performed, but should like to understand the system properly.—Hesketh. [Will, before long, give a simple account of the matter.—Ed.]

[121]—Latitude and Longitude.—I am anxious to be able to determine the latitude and longitude in a practical manner. Will you kindly, at your earliest convenience, give me the necessary instructions in Knowledge?—Hesketh. [The best advice we can give "Hesketh" is to obtain Loomis's excellent treatise on "Practical Astronomy," in which the best methods of determining longitude and latitude are clearly and fully explained.—Ed.]

[122]—Blood Analysis—Is there any method by which the amount of the albumen, or, at least, of the saline constituents of blood, can be estimated quantitatively, in cases where only a small quantity can be obtained, say 5 c.c.? Can the spectroscope be applied to quantitative analyses in such cases?—*ἄμα*.

[123]—Fermentation in Beer.—Will you kindly allow me to ask if any of the readers of Knowledge will explain to me the process of fermentation in beer?—In Re.

[124]—The Coffee Leaf.—Have you heard anything lately of any experiments made in this country to introduce the coffee leaf among us? My reason for asking is that I think our labourers would derive a great benefit from its use, if it could be introduced.—F. C. S.

[125]—Indigo.—Is it possible to prepare indigo on a large scale artificially, and at the same time profitably? I have heard rumours that there is a manufactory in France for doing so; but have looked through, as they have arrived, my society's journals to see if I could find an account of the experiment, if any has been made, but it has been without success. If you could enlighten me on the above you would oblige.—F. C. S.

[126]—Flora, and Plants, and Insects of the Channel Islands.—Why are the plants of the Channel Islands included in the British flora, while the insects are usually considered French?—B. J.

[127]—Double Refraction.—I should be very glad indeed of information upon the following points:—When a spot of light is viewed through a piece of Iceland spar, two spots are seen, as a consequence of the double refraction of the crystal; and if I understand rightly, one of them consists of vibrations, say, in vertical, and the other of vibrations in horizontal planes. Now, if we look at these through a second piece of spar, we see four spots of light, each of the former pair being doubled or "resolved." What I want to know is the meaning of "resolved." If one of the first pair consists of vertical vibrations, where do the horizontal ones come from to form its second image? and if the other consists of horizontal vibrations, how do we come by the second image? Each pair behaves, when the spars are revolved, in the same manner as the first pair would if seen through a Nicol prism.—Polar. [It depends how the second piece is held. If it is held with its principal section parallel to the other's, only two images are seen, one by ordinary, the other by extraordinary refraction. The same if the principal sections are 180° from each other, unless the crystals are of equal thickness, when there is but one image. If the principal sections are at right angles, only two images are seen, which are (1) the ordinary image after extraordinary, and (2) the extraordinary after ordinary refraction. In intermediate positions, four images are seen, because neither the ordinary nor the extraordinary refracted rays would have, in order to pass through, to turn through quite a right angle, which, and which alone, would involve total extinction.—Ed.]

[128]—The Eye as one of the Senses.—I wish to give an essay, or a reading on this subject at a Working Man's Mutual Improvement Society. I should be extremely obliged with any information that would enable me to get a cheap pamphlet, or anything that will help me to prepare such an essay on "The Eye, one of the Gateways of Wisdom."—T. T.

[129]—Medical Botany.—Will you kindly tell me through your valuable paper the name of the best medical botany, with diagrams.—Henry Hawkes-Spink.

Replies to Queries.

[20]—Orrery.—In the Museum of Philosophical Apparatus, at University College, Gower-street, London, was, and probably now is, the Orrery made by the celebrated self-taught astronomer, James Ferguson.—W. H. Hardy.

[66]—Venus Casting Shadow.—In India I have repeatedly seen Venus at least two hours before sunset. In 1849 or 1850, while driving on a moonless night, we perceived on the road a distinct, though faint, shadow of a clump of trees, of two large poles, &c., cast by Venus. This was not far from Delhi, in a very dry, clear atmosphere.—Cogito.

[72]—Antiquity of the Pyramids.—Notwithstanding Sir John Lubbock's statement, there is no trustworthy evidence that the Pyramids "are at least 6,000 years old." The Great Pyramid of Geezeh is generally admitted to be not only the greatest, but also the oldest, and astronomical considerations based upon the position of its entrance-passage—which constitute evidence of a much more reliable character than that of ancient Egyptian traditions—indicate that its age does not exceed 4,051 years.—J. Baxendell.

[Mr. Baxendell fails to notice that the direction of the entrance-passage fulfilled the condition of pointing towards Alpha Draconis at its sub-polar passage, at two epochs during the last 25,000 years—one that which he mentions, the other some 6,000 years ago. Moreover, what no one seems to have noticed yet, the ascending passage, which 4,051 years ago would have been directed towards no important star, would have been directed towards the most interesting orb in the whole star-sphere—namely, Alpha Centauri—at its southern culmination, at the earlier epoch. It has been supposed by persons unacquainted with astronomy that the Pleiades occupied the corresponding position 4,000 years ago. But this is not the case. The relation indicated by Prof. Smyth was only a symbolical, not an observational one.—Ed.]

[83]—Chemical Queries.—In reply to "Theion" (*a*), there are several allotropic modifications of sulphur, as the following table shows (Valentin's "Introduction to Inorganic Chemistry, p. 53):—

	Sp. G.	Fusing Point.	Solubility in CS_2.
1. Octahedral.	2·05	115	Soluble.
2. Prismatic.	1·98	120	Insoluble before transformation.
3. Plastic.	1·95	Are converted into the octahedral modification.	Insoluble.
4. Amorphous.	1·95		Insoluble.

Another modification insoluble in CS_2 is contained in flowers of sulphur, and is of a light yellow colour (Roscoe and Schorlemmer's "Treatise on Chemistry," vol. i. p. 292). If "Theion" will carefully read Roscoe's "Lessons" again, he will find that the plastic modification, while *tenacious*, is insoluble in CS_2. When the tenacious form has become *hard* and *brittle*, a portion will dissolve in the CS_2, and leave a dark-brown powder ("Treatise," vol. i. p. 292). Miller and some other chemists call this a grey amorphous powder. This latter modification corresponds to No. 4 in the above table. (*b*.) The word *nitrous* should be *nitric*. It is stated correctly in the "Treatise on Chemistry," vol. i. p. 426.—C. W. D.

[84]—Ancient Man.—Mr. Horner's researches were published in the "Philos. Trans.," 1855 and 1858, and reviewed in the *Quarterly Review* in 1859. His argument for an extreme antiquity of Egyptian civilisation from finding fragments of pottery at considerable depths in the alluvial soil is now considered insufficient. Sir C. Lyell says, "it is not worth while to notice such absurdities." Other pieces of Greek origin have been exhumed at greater depths, and I believe the Greek honeysuckle ornament was found on some of Mr. Horner's. Stephenson turned up a brick of Mehemet Ali's, even at a lower level. Stone implements from Egypt have frequently been exhibited at the Anthropological Institute, by Sir J. Lubbock in 1874, Capt. Burton in 1875, and General Pitt Rivers in 1881, but with regard to many of these, there is great doubt as to whether they are hand-fashioned, and even those which are, do not prove the existence of a stone age in the Nile Valley, for Prof. Owen says, "Chert, chipped to an edge, or flint flakes struck off by percussion, being the ordained materials for circumcision and for the abdominal cut in mummifying, the finding of flint knives in Egypt requires evidence of the date when they were used, or of previous manufacture, before they can apply to the question of a

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race anterior to the historical life of the country." Mariette says, "the use of stone and flint tools extended nearly 4,000 years, through thirty dynasties." Brugsch tells us, "Egyptian history throws scorn on the supposed periods of stone, bronze, and iron;" and Chabas shows that similar tools were in use at the latest historical period, and even now are not uncommon among the Arabs. —A MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY.

[84]—ANTIQUITY OF MAN, AS SHOWN BY THE WORKS OF ART IN NILE MUD.—Your correspondent "Clio" may find Mr. Horner's researches in the "Philosophical Transactions," 1855-8, or perhaps she will do better by consulting an admirable summary in Lyell's "Antiquity of Man," p. 35 and 41, 4th edition, 1873. The result is a simple rule of three sum. As is the thickness of Nile mud deposited by the annual inundations over certain historical monuments to their known age, so is the greatest depth at which the works of art occur in like mud hard by to their approximate age.—A TRAMP.

[89]—COLLODION PLATES.—"Persevere" does not say whether the red stains appear after the plate is developed, or whether they are on the plates as received from the maker. In either case, it would be almost impossible to determine cause without inspection. The vagaries of gelatine plates are innumerable. If the negatives are not varnished, they are very liable to be spoiled by the silver from the paper; the gelatine readily absorbs moisture. The remedy is obvious. Evidently "Persevere" has not tried to answer his own question as to developing gelatine plates; let him try, and give his experience, if successful.—A. BROTHERS.

[91]—"MISSING LINK."—Dr. Andrew Wilson's article is on "Missing Links" (not on *the* "Missing Link"). It appeared in the *Gentleman's Magazine* for September, 1879.—ED.

Notes on Art and Science.

AMONG recent finds at Pompeii were several amphoræ, on some of the largest of which was written the exact date of the extraction of the wine contained within, and on smaller ones the names of the wine.

IT is announced that Dr. King, in charge of the Government cinchona factory at British Sikkim, has succeeded in manufacturing, for the first time in India, sulphate of quinine from local cinchona bark. The samples produced are said to bear comparison, on analysis, with the pure sulphate of quinine of commerce, and preparations are being made for undertaking the manufacture on a large scale.

THE famous spring of boiling water in the middle basin of Hell's Half Acre, in the Yellowstone region, has lately become still more wonderful as a geyser. Four or five times every twenty-four hours it discharges a great column of water, freighted with stones and obscured by a dense volume of steam. The hollow formation for hundreds of yards around the orifice trembles under the upheaval, and the water is thrown to a height of a hundred feet.

THE six healthiest cities of the United States are said to be in the order following:—Utica, Dayton, New Haven, Portland, San Francisco, and Lawrence. The unhealthiest are Charleston, Memphis, Cleveland, Chicago, and Lynn. St. Petersburg is the unhealthiest city in the world, and is followed by Charleston, Malaga, Alexandria, Warsaw, and Buda-Pesth.

M. MACAGNO, in *Les Mondes*, states that he has been making experiments on the influence of electricity upon the growth of the vine. An electric circuit was formed by copper wire between the extremity of a branch bearing fruit and its origin near the soil. More wood was formed in the branch, which contained less potash than the other parts, and the grapes ripened more readily, containing an excess of sugar.

IT is said that a marked improvement has been noticed in the acoustic properties of the Grand Opera House, Paris, since the introduction of the electric light. A layer of heated gases acts as a screen for sound, hence the volumes of hot fumes arising from the old gas footlights obstructed and marred, to some extent, the voices of the singers. With the electric light, inclosed in air-tight bulbs, no fumes can be emitted, and very little heat is given off. Hence its benefits to the ear as well as to the eye.—*Frank Leslie's Magazine.*

THE question of the existence of volcanoes in Central Asia, especially on the Kuldja frontier, has always been a matter of doubt and discussion among geologists and Russian explorers. The Governor of Semfretchinsk, General Kolpakofsky, had already fitted out expeditions to settle the question—one in 1878, and again in 1879; but owing to the difficulties of reaching the mountains, which the Chinese considered impassable, and also to disorders which were then taking place in Kashgar, both expeditions were unsuccessful. This year General Kolpakofsky again set himself to the task, and now reports that he has at last discovered the perpetual fires in the Thian Shan range of mountains. He telegraphs that the mountain Bai Shan has been found twelve miles north-east of the City of Kuldja, in a basin surrounded by the massive Ailak Mountains, and that the fires which have been burning there from time immemorial are not volcanic, but proceed from burning coal. On the sides of the mountain there are caves emitting smoke and sulphurous gas.—*Frank Leslie's Magazine.*

Our Mathematical Column.

THE USE OF LOGARITHMS.

IN our last, we took a simple case of the multiplication of two numbers, each of six digits, and the division of the products by another number, also of six digits. Working this sum by logarithms seemed rather long, just as in practice nearly every one finds the first two or three sums he works by logarithms require more care and watchfulness than he afterwards finds necessary. We may now, however, proceed more freely.

Let us consider a few cases of taking powers, or extracting roots of numbers.

Take first the familiar problem of the horse with 24 shoe-nails, for which a price of ¼d. for first nail, ½d. for second, 1d. for third, 2d. for fourth nail, and so on, doubling to the twenty-fourth nail, was to be paid: to find his price by the aid of logarithms. This is a case somewhat unlike those usually dealt with, where an answer exact to so many decimal places is required, not an answer absolutely exact. However, it can be readily solved by logarithms. Thus, for the first nail, amount is ¼d.; for first and second, ¾d.; for first three, 1¾d.; for first four, 3¾d. Start from the fifth, for which nail 4d., or a third of a shilling, was to be paid, and let the third of a shilling be our unit (the reason being that if we take pence or farthings, we should not be likely to get an exact result). Then there is to be paid for fifth nail 1, for the sixth 2, for the seventh 4, or 2 to power 2; for the eighth 8, or 2 to power 3; and so on: and for the twenty-fourth, 2 to the power 19, while we know that the total paid for all the nails from the fifth to the last is twice this, less 1, or $2^{20}-1$.

Now, turning to the tables we find—

log. 2 = 0·3010300
Multiply by 20 — 20

log. 2^{20} = 6·0206000 = log. 104856

log. 10485 = ·0205684

316
difference for 70 = 292
240
difference for 6 = 250

This part of the calculation is simply working out the logarithm.

We know that 2^{20} must be a whole number, and can end only with one of the digits 2, 4, 6, or 8; so we take with confidence the number 1048576. This, less 1, is the number of 3rds of a shilling for the nails from 5th to 24th inclusive, and the first four give us one-third of a shilling, less a farthing. So the answer is 1048576 thirds of a shilling, less a farthing.

Or 349,525s. 3¾d.
Or £17,476. 5s. 3¾d.

This is an unfavourable example, because of the necessity for an exact instead of an approximate result. If we had only required to know the amount roughly, that is, within a shilling or two, we might have proceeded thus:—The amount in farthings is $2^{24}-1$, or in shillings the amount is $\frac{2^{24}}{48}$, neglecting the farthing.

Now log. 2 = ·3010300
log. 2^{24} = 7·2247200
log. 48 = 1·6812412

log. $(2^{24}+48)$ = 5·5434788

log. 34952 ·5434720
difference for 50 62
5 6

5·5434788 = log. 34952·5
Ans. = 34952·5 shillings.
= £17,476. 5s. 6d.; and the conditions of the problem show that the pence really amount only to 3¾d.

In nearly all problems requiring the use of logarithms, however, we do not require exact accuracy, but may be content with approximation to the third or fourth place of decimals.

Let us take a case not scientific, but practical.

Required the amount of £1,828 at compound interest, five per cent. per annum (payable yearly), at the end of ten years.

Any sum at the given rate of interest, is increased in the ratio $\frac{105}{100}$ at the end of the first year; therefore, at the end of two years, it is increased in the ratio $\left(\frac{105}{100}\right)^2$; and so on; and at the end of ten years it is increased in the ratio $\left(\frac{105}{100}\right)^{10}$. Thus we have to find the value of the following expression:—

$$1828\left(\frac{105}{100}\right)^{10}$$

Now log. 1828 = 3·2619762
Ten times log. 105 = 20·2118930
Sum = 23·4738692
Ten times log. 100 = 20·
Log. (answer) = 3·4738692 = log. 2970·771

There is an error in the addition, log. (answer) = 3·4738692. True answer somewhat greater than stated.

20
15,420
12
5,040

Answer is £2,970. 15s. 5d.

Here is another question relating to compound interest:—In what time will a sum of money, say £100, be doubled at 5 per cent. per annum, payable yearly, compound interest?

If x be the required number of years, we have

$$(100)\left(\frac{105}{100}\right)^x = 200$$

or $105^x = 2 \times 100^x$

This is the same as saying that

$$x \log. 105 = \log. 2 + x \log. 100$$

or $(2{\cdot}0211893)\ x = 0{\cdot}3010300 + 2x$

$$\therefore \quad x = \frac{3010300}{211893}$$

We can again use logarithms to determine the value of this fraction.

We have log. 3010300 = 6·4786098
log. 211893 = 5·3251167

difference = 1·1524931 = log. 14·207

12

2·484

Answer is 14 years 2½ months, very nearly.

For greater exactness, multiply ·207 by 365, giving 75·555, and making answer, 14 years, 75 days, 13 hours, 19 minutes.

The student of our subject is advised to go carefully through each computation. He will note that in taking out the logarithm of 211893, we put down for the first three figures not 325, but 326, though the logarithm is found in a section which seems to have 325 for its leading digits. In this section we find 325 followed by numbers continually increasing up to 9875; then on the same line comes $\overline{0}080$, which, of course, means that the logarithm has increased from 3259875 to 3260080. The student must be careful on this point, especially in the earlier part of the tables, where the changes are more rapid.

One other example illustrating an application of logarithms, in which great time is saved.

Suppose we want to find the cube-root of a number, say, 21,793, correct to the third or fourth decimal place. By the ordinary arithmetical process this would be a long job, and we should have carefully to test the result to insure accuracy. But by logarithms the process is very easy, thus:—

Log. 21793 = 4·3383170
Divide by 3 giving 1·4461057 = log 27·93224.

Thus the cube-root of 21793 is 27·93224.

Take a more complex case, the solution of which by ordinary arithmetical processes, with the same degree of accuracy, would take half-a-day at the very least, even in the case of an arithmetician knowing how to take out the fifteenth and seventh roots of numbers.

Find the value of the expression:—

$$\frac{(1828)^{\frac{2}{15}}(0{\cdot}1763)^{\frac{1}{3}}}{(745)^{\frac{1}{7}}(0{\cdot}0051)^{\frac{1}{4}}}$$

log. 1828 = 3·2619762
2-15ths or 4-30ths of this = 0·4349302
log. 0·1763 = $\overline{1}$·2462523
1-third of this, or of $-3+2{\cdot}2462523$ = $\overline{1}$·7487508

0·1836810 (A)

log. 745 = 2·8721563
1-seventh of this = 0·4103080
log. 0·0051 = $\overline{3}$·7075702
1-fifth of this, or of $-5+2{\cdot}7075702$ = $\overline{1}$·5415140

$\overline{1}$·9518220 (B)
A − B = 0·2318590
Answer = 1·705529

The student will notice how the negative characteristics are dealt with in such problems. We must always add enough to the negative characteristic to make it exactly divisible by our divisor, treating the number thus added as a positive characteristic for the rest of the division.

Our Chess Column.

GAME recently played at Simpson's Divan between Mr. A. P. Barnes, of New York, and Mr. Gunsberg.

Queen's Gambit declined.

White. Mr. Barnes.	Black. Mr. Gunsberg.
1. P. to Q.4.	P. to Q.4.
2. P. to Q.B.4.	P. to K.3.
3. P. to K.3.	Kt. to K.B.3.
4. P. to Q.R.3. (a)	P. to Q.B.4. (b)
5. P. takes P.	B. takes P.
6. P. to Q.Kt.4.	B. to K.2. (c)
7. P. to Q.B.5.	Castles. (d)
8. Kt. to K.B.3.	P. to Q.R.4.
9. B. to Q.Kt.2.	P. to Q.Kt.3.
10. P. takes Kt.P. (e)	P. takes P.
11. B. to K.2.	P. takes P.
12. B. takes P.	B. takes B.
13. R. takes B.	R. takes R.
14. Kt. takes R.	Q. takes P.
15. Castles.	B. to R.3.
16. Q.Kt. to Q.Kt.5. (f)	Kt. to K.5. (g)
17. Q. to Kt.3.	Kt. to Q.2.
18. R. to Kt.sq.	R. to Kt.sq.
19. Q. to R.3.	Kt. to Q.3. (h)
20. Q. to Kt.4.	B. takes Kt.
21. B. takes B.	Q. takes B.

White resigns.

NOTES BY "MEPHISTO."

(a) This is preparatory to advancing the Pawns on the Queen's wing. We cannot approve of such a course with all the White pieces yet undeveloped.

(b) In most openings, where the first player opens up the Queen's wing first P. to Q.B.4 (to be followed, if feasible, by Kt. to Q.B.3) will be found effective, as it attacks the centre pawns, which threaten to domineer over Black's game.

(c) The Bishop is sometimes withdrawn to Q.B.2 *vid* Kt.3, the idea being that on B.2 he is available for attack on the King's side (this is problematical). We prefer B. to K.2, for, in the first instance, it affords some protection against B. to Q. Kt.2. Secondly, we consider the hostile Queen's wing weakened, and from K.2 the Bishop will render assistance in attacking the White Pawns successfully (this is positive).

(d) Necessary before beginning the attack. Many good games are often thrown away through rashness in attack and insufficient regard for one's own safety.

(e) There is nothing better: if 10. P takes R.P., then 10. P. takes B. P., and the Rook's Pawn is lost; or if 10. Q. to B.2. 11. P. takes B. P. / P. takes B.P. / Kt. to K.5.

(f) If B. takes Kt., then Q. to B.3. would win the piece back. White intended to bring his Knight to Q.4, but it would have been much simpler to have brought him *vid* B.2.

(g) Taking the proper advantage of White's weak move, Black threatens to win a piece by B takes Kt., or, if the Knight retires, by Kt. to B.6.

(h) This ends the struggle. Black now wins the Knight, for if 20. Q. Kt. to B.3, then 20. Q. takes R., and the White Knight cannot take the Queen on pain of mate in 4 moves. If 20. K.Kt. to Q.4 then P. to K.4. wins.

Our problem No. 4 (wrongly numbered 3) in No. 5 is solved by R. to Q.R.8.

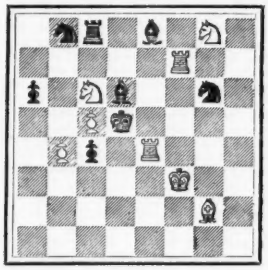
We have received correct solutions from Gamma, Arkansas, E. F. K., Caissa, A Young Player, Try Try Again, S. D. P., R. M., Afternoon, Worcester, Etoniensis, D. Sec. Others incorrect.

Edward Sargant points out that Mr. Healey's problem is unsound. If black Pawn becomes a Rook, white cannot win. This is so. The point appears to have been noted in "Westminster Papers" several months later. By putting the black Rook's Pawn one square further forward the flaw is corrected.

From the *Glasgow News*.

By Mr. C. R. Baxter, Dundee.

BLACK.



WHITE.

White to play and mate in three moves.

This is the problem to which we referred in No. 6. Mr. Baxter tells us that he had never seen our older problem. We had not supposed he had. Next week, or later, we shall give an instance in which we were anticipated. He notes the resemblance of Carl Eggert's problem, in *Illustrated London News* for November 26, to his own, which appeared the same day.

Our Whist Column.

BY "FIVE OF CLUBS."

H. P. H. points out a mistake in our discussion of this matter. Lord Y. should have wagered 1,827 to 1, not 1828 to 1, the chance being 1-1828th, and the odds, therefore, 1,827 to 1. Of course "H. P. H." is right. The numbers representing the odds and the chance are so nearly the same in such a case as this, that we were not careful about a point which in others of our papers on chance we have insisted on clearly and often. In dealing with another point, "H. P. H." misconstrues us. He says, "the chance that a Yarborough will not happen in any deal is *not* the same as the chance that it will not happen in a given hand in four successive deals; for in the former case one hand depends on the other to a certain degree, whereas in the latter, the chance of any combination happening is quite independent of any combination which may have preceded it. I agree that the chance of a Yarborough is $\frac{1}{1828}$, and consequently the chance of a Yarborough in four consecutive deals is $\frac{1}{1828^4}$. Following your principle, this would be the chance of four Yarboroughs in one deal, which is a manifest absurdity, for we ascribe" (thus) "a mathematical chance to a clear impossibility." The question we were really considering was what odds should be offered to each member of a party of four at whist that his hand would not be a Yarborough; and we (practically) affirmed that £1,827 to £1 should be offered to each. "H. P. H." seems to consider that this is the same as offering £457 to £1 (roughly) against the occurrence of a Yarborough in a single deal. But this is not the case. Take a simple case illustrating at once his difficulty and our position:—

Suppose there are four cards marked respectively A, B, C, D, to be dealt, one to each of four persons. Then the chance that any particular card, as A, will be dealt to any given person of the party of four, is obviously one-fourth, or the odds 3 to 1 against that event, so that with that person any one might at once safely and honestly wager £3 to £1 against his getting that card. Now our position is that the same odds might be offered with each one of the four, although it is certain, in this case, that some one of the four must have card A. (In the Yarborough case it is not certain but more probable that one of the four will have a Yarborough than that any particular one will have such a hand.) Well, "H. P. H." might reason that this is not the case, for if the chance is $\frac{1}{4}$ that a particular person will have card A in any given deal it is $(\frac{1}{4})^4$ that he will have it in four successive deals, and on our principle the same is the chance that each one of the four persons will have the card A in a single deal, or, in other words, the odds are only 255 to 1 against the manifest impossibility that each member of the party of four shall have the same card dealt to him out of four. Yet it is perfectly clear that the just odds are 3 to 1 with each person of the four, and the proof is that if these odds are wagered with each the event can bring neither gain nor loss to the layer of the odds; he will have to pay £3 to one of the four, and receive £1 from each of the others. So, if a person wagered £1,827 to £1 with each of four persons, before an ordinary whist deal, that that person would not get a Yarborough, he would be laying the just odds. Now let us see what his wager really amounts to in this case. If he loses to one, he loses £1,827. One of the others might have a Yarborough, but the chance that this would happen is very small: it is really this, that out of thirty-nine cards dealt to three persons, one would only receive cards belonging to a particular group of nineteen—a chance very small indeed. Regarding it for the moment as zero, we may say that it is certain, or all but certain, that from each of the remaining three players the layer of the odds will receive £1. Therefore, the layer of the odds pays £1,827 and receives £3, or loses only £1,824. His case is, therefore, similar to that of one who had laid against a Yarborough occurring in each of four successive deals to one only of the four players, except that this one might have had to pay £1,827 for each of the four deals, whereas the other could only have to pay for two at the outside, and would most probably have had to pay for one only. The difference exactly makes up for the interdependence of the four hands in any given deal.

Take a simpler illustrative case to show what we mean. A person, P, wagers with another, X, one of four to whom four cards, A, B, C, D, are dealt four times running, that X will not receive a particular card A, offering £3 to £1 at each of four deals. Unquestionably each wager is fair. X may have A each time, in which case P will have to pay £12, or X may not draw A at all, in which case P will receive £4. There are other eventualities easily followed. But the wager is manifestly fair. Now take a single deal. P wagers with W, X, Y, Z severally £3 to £1 that they will not have card A. In this case, one of the four must have the card, and to him P must pay £3, receiving from each of the others £1, or neither losing nor gaining. Since each wager, or rather each set of wagers, is manifestly fair, we see that the possibility in such cases of having to pay the odds more than once when successive deals are considered, exactly counterbalances the certainty of winning in some cases (most probably in three when a Yarborough is in question, and certainly in three where four cards only are in question), in the case of wagers with the four parties to a single deal. We have, in fact, only to ask whether a certain wager with one party to the deal is fair or not. If it is fair, we may be well assured that there is no unfairness (either way) if the same wager is made with each of the four players.

However, although this, and this only, was what we were considering (as should be obvious from our remark beginning, "Supposing Lord Y. offered £1,000 to £1 to each member of a whist party for ten deals," &c.), "H. P. H." very naturally misunderstood us, seeing that we wrote, carelessly, as if we were considering "the chance that a Yarborough will not occur in any given hand" (these are our very words, and naturally misled him). The chance of this is not to be inferred so simply, as our words might have suggested, from the true odds against the occurrence of a Yarborough in a single hand. To return to our simpler case. The odds against a card A being drawn by one of the four is $\frac{1}{4}$, and the true chance of its being dealt *once* to a given person in four successive deals is $(\frac{3}{4})^4$ or $\frac{81}{256}$; so that the odds in favour of its being dealt to him once in four deals are 175 to 81. Thus only £81 can be safely wagered against £175 that the card will not be dealt once, at least, in four trials to one of the four players; but the chance of the card being dealt to one of four persons in a single deal is, of course, certainty, or 1; so that no sum, however small, can be wagered against any sum, however large, that the card will not be so dealt.

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Answers to Correspondents.

*** *All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

A "PARADOX-MONGER." Very likely you are right; with the kindest wishes and the best intentions men make enemies. It will be unfortunate if our evasion of any particular subject should be attributed to "predjudice"; but we *must* omit some things, and amongst those your letter.—S. H. WRIGHT. You seem to me entirely to misunderstand Dr. Wilson's first letter—it was explanatory, not controversial. The spider's web is not a greater difficulty than the cell-making instinct in bees; the study of Darwin's reasoning on the latter subject would show you that there is no real objection in the arguments you consider valid against the Darwinian hypothesis. If you remember that of five letters received four *must* be omitted, you will see that you have no real reason to complain that your former letter was not inserted. It would seem that we are bound to offend four-fifths of our correspondents if we keep open a correspondence section at all.—ROBERT KIDD. The point has been corrected already.—PASCAL. You are, of course, right; but is it worth while to reason back so carefully to the original equation?—G. B. STUBBS. Thanks. Our Chess Editor and "Mephisto" will analyse it.—J. JEFFARES. In his reply to Archimedean, p. 61, "Mathematicus" took it for granted that the former would at once see what you have established, and what H. A. N. says the differential calculus is required to establish. As you say, the substance of the proof is to be found in books; we *must* not find space for it.—F. S. PILKINGTON. The experiment proving that one part of the retina is insensible to visual impressions is well known. Must reserve space for newer matter.—JAS. H. SYMINGTON. Letters relating to Vivisection will receive due attention, but the matter is hardly worth discussion; it seems so clear that those who hold extreme views on either side are mistaken. The editor of the paper you mention is well known to be an extreme opponent of vivisection, one who would not allow a tom-cat to have a hair plucked out if the lives of all his relatives depended on the experiment. On the other hand, there are some who would let a thousand dogs be tortured to death to establish a physiological law of no benefit to a living creature. Fanatics of the first sort consider all who approve of any experiments whatever on animals to be brutes, while those of the second sort consider all who oppose the wantonest cruelty, to be drivelling idiots. No amount of discussion will reconcile these, or either sort, with the moderate persons who hold that researches really directed to benefit the human race may be conducted at the expense of pain to animals, provided that every means be adopted to reduce such pain to a minimum. Your story hardly establishes the intelligence of that particular collie. What did he gain by taking the chop under the sofa, and returning it uneaten? Are we to assume that he was planning how to get the bone out without injuring the chop? If he had done this it would have shown intelligence; as it was he only showed the effect of craving.—R. S. COUCH. You will find a reply to some of your questions in "Found Links," presently to appear in KNOWLEDGE.—H. D. General information on the subject of your letter will be given in numbers beginning December 30, or January 6, at latest, in a series of papers by a Fellow of the Royal Astronomical Society, on work with a 3-inch telescope. Most of the objects he will consider will be perfectly well suited to your telescope, though it is of only 2½ inch aperture.—F. J. BRODIE. We cannot at present make any arrangement: probably we may follow a plan which we pursued some eight years ago, when editing the natural science department of the *Mechanic* (not the *English Mechanic*, but the older paper, now merged in *Iron*).—JOHN BENNETT. Thanks. You will see that the "Witch of Agnesi" has been fully dealt with; the geometrical instructions being better suited to these pages than the analytical methods, though not so easily planned. The dotted curves in your figure, I see, correspond with the equation when $x-2$ *a* is written for $2a-x$.—A CONSTANT READER AND SUBSCRIBER. It is not the case that the moon never sets in the polar regions and is never seen in the crescent form. The moon is only well seen as a full moon in the arctic winter months, and when so seen does not set. The crescent moon can be seen in the day time in the arctic summer months, not at all in the winter months.—J. A. R. I fancy I have seen your views respecting comets before. Was it not in the *English Mechanic*? They seemed to me unsound then, and I must confess I find no reason now to view them more favourably. They are inconsistent with many known facts.—ROB. GODFREY. Thanks. Have you heard the family anecdote about R. C.'s eleven reasons for not acknowledging the work?—COGITO. The trisection described by Mr. Warren may be extended to any angle by first bisecting, then trisecting the half angle. It is impossible by Euclidian geometry to draw from C a line CED such that DE shall be equal to OE.—J. BACON. You are quite right. Sir D. Brewster wrote when the true nature of the spectrum was not understood.—J. M'GREGOR ALLAN. Many thanks. Your interesting discussion of Section 3 shall presently appear.—C.T.B. Hardly room for Gems at present.—G. M. The method of trisecting an angle described in Mr. Warren's letter will not do for angles above 120°, simply because when the angle 120° is reached, the point D comes up to the circumference of the semicircle.—A. J. You do not seem aware of the fact that the precession of the equinoxes is not explained empirically by the modern theory, but is shown to be an inevitable consequence, in measure and degree, of the action of gravity. It is one of the most beautiful of the proofs of the Newtonian theory. Your theory would be all right, if, first, it were certain that there would be no such effects as the law of gravity assures us there must be; and, secondly, if we knew that the sun were circling in a mighty orbit, such as you picture, in 25,900 years or thereabouts. As we are quite certain the sun is not so moving, and equally certain that the earth being spheroidal must reel precessionally precisely as she does, your theory cannot be satisfactorily maintained.—J. T. E. Mr. Foster has your illusion in his collection. It was published thirteen years ago in the *Intellectual Observer*. Mr. Foster complains that the increase of correspondence is destroying all his illusions.—K. SYMONS. Mere extravagance.—EDWARD COX. You are quite right; but proving the earth not flat is killing the dead. The argument you use has been employed often—by myself among others,—but the Flat men cannot or will not see what to persons of average brain power is so obvious.—THOMAS BOUTTELL. You misunderstand Dr. Wilson's reply to "Ignoramus," whose "frankness," you say, "brought down the wrath of his opponent." There is no trace of warmth, far less of wrath, in Dr. Wilson's letter. The tone of yours is, therefore (you will admit, I think, on consideration), unsuited to the occasion. Your letter is also not sufficiently condensed. If you had read Dr. Wilson's excellent essays on evolution, you would be aware that what you desire to "wisper in his tympanum," would be very far from being news to him. The evidence you adduce to "vanish the phantom touching those rudimentary members which puzzles so many of us," is precisely the evidence which the evolutionist quotes in support of his position. In any case, the lecturer on Zoology and Comparative Anatomy in the Edinburgh Medical School, does not need to have such well-worn knowledge whispered in his ear.—T. SMETHURST. We hope to give, before long, such articles on Time as you desire.—COGITO, C. E., ALGOL, &c. Thanks; questions already answered.—W. H. JACOB. The explanation is simple. Your right eye being very short-sighted, takes no part ordinarily in the vision of objects beyond its focal range. When you close it you see such objects (your finger and the distant light, for instance, in the experiment you describe) just as you did when it was open. But when you close the left eye the right eye is at once called into action. You see the distant object and your finger with this eye, though not well defined. Moreover, you see your finger to the left of the light, because the line from the right eye to the finger must obviously fall to the left of the line from the same eye to the more remote object (this object, the finger, and the left eye being in a straight line).—THOS. HERDEN. Thanks. It shall appear early.—W. GR. ROLFE. Your article on Seismology comes at an inconvenient time, when two papers on the subject are already in type.—H. EDMONDS, B.Sc. We could hardly do so without apparently recommending that of which we have no personal knowledge.—HARRIS. Loomis's "Treatise on Practical Astronomy" is one of the best.—J. H. G. If there were any recognised connection between mild Novembers and the winters following we could ask your question, but there is not.—J. MITCHELL. Thanks; but no room. Clio's query already answered. Mariette, R. Lenormant, and Wilkinson quite out of date, Bunsen nearly so. Your question about first infant, and how it fed, reads like a joke.—VEGA. Question 65 now sufficiently answered.—EXCELSIOR. We were referring to another. Have no knowledge of the instrument you mention. Why not write to makers, asking what guarantees they can give as to performance, &c.—W. C. BLYTHE. Thanks; your interesting letter shall appear. PROF. LUNGE. Thanks.—CHAS. MOULTON. Your idea submitted to publishers; but it seem so easy for readers who prefer it to put the stitch through the paper for themselves.—D. K. Your idea that the sun-spots may be intra-mercurial planets was the first which Galileo, Fabricius, and Scheiner entertained (and disposed of) when the spots were discovered 271 years ago.—F. W. DILLON. The cat who put her paw on your knee when you were eating, so that she might receive a share of your attention and of your supper, showed intelligence, no doubt; but the case is not so unusual as you seem to think.—ERNEST C. A. AXON. Thanks.—THOMAS NEWBUTT. Thanks. Query now sufficiently answered.—T. R. Regret that we have no space at present for your sound discussion of the moon's rotation.—CUPIDUS SCIENTIÆ. Puzzle marked for insertion early.—A. J. MARTIN. The eyepiece of a telescope is a microscope, by which the image formed at the focus of the object-glass is magnified.—TENTATIVE. We cannot admit the ordinary stories of spiritualism, but anything tested by due experiments, properly conducted and vouched for, would, of course, be in our line.—MEMB. SOC. BIBL. ARCH. Many thanks for your interesting letter. We slightly alter one or two passages, lest they should lead to rejoinders of an undesirable kind.—CÆSAR. The letters S. P. Q. R. were used on Roman banners, &c. They stood for Senatus Populusque Romanus (the Senate and People of Rome).—WM. YOUNG. Your suggestion is a good one, but at present something more than the name seems wanted.—A. A. YOUNG. Questions sufficiently answered. D'ARTAGNAN. "Engraver" wants more special instructions.—J. A. R. Letter on rainbow presently, if can make space, but the proof belongs to text-book matter.—J. F. SIMPSON. Many thanks. Correspondence occupies so much time and space that other matters, which I regard as more important, stand a chance of being neglected, unless a very strict hand is kept on our correspondence column.—ALPHA. Certainly, "would" and "could," for "will" and "can," are suggestive of Mrs. Brown.—H. E. PARKER. Chemical question answered sufficiently.—E. F. B. HARSTON. It is easier to explain than to name suitable book. You only get the force retardative by carrying the body outside the circle. So long as it moves in the circle, the force always act at right angles to the direction in which the body moves, and therefore neither hastens nor retards it.—A HAVILAND. Will see about it.—C. J. SHAW. Thanks. Queries already answered.—SIGMA. The illusion not striking in the picture, which would cost a good deal to engrave.—CHAMPION. The advertisement may or may not involve a reference to KNOWLEDGE. At any rate, we claim no rivalry in one of the subjects named. The last thing we should think of would be to assert any claim as to the other. We must let readers decide.—ORION. We cannot see that there is any difficulty, even now; later, the numbers will be widely different. Enough has been said about grammatical question, we cannot but think.—SONNET. Our heart is hardening rapidly.—W. ARTHUR. How many have other knowledge than from books on the subject named: and if those who have are inarticulate, what good would they do our readers.—AN OLD BOY. We quite agree with you; Cassell's *Boy's Newspaper*, and S. Low & Co.'s *Union Jack* are capital.—F. C. S. Your proof shows how an angle of 135° may be trisected; but considering that such an angle is equal to the half of three right angles, or to three half right angles, this can hardly be regarded as the solution of the general problem, which is what is wanted. The other angle trisected in your proof is the complement of 22½ degrees, or 67½ degrees, the third of which is one-fourth of a right angle. KNOWLEDGE should reach Liverpool earlier than 1.30 p.m. on Saturday.—A. A. MCMASTER. You are certainly right. The length of the rope makes no difference if the action on the weight is along the rope. Great width between the axletrees would make a difference, because part of the strain on each side is parallel to the axle, and, of course, wasted.—F. J. D. SELBST. Will keep your remarks about water spheroids for further consideration.—M. D. P. Will try to find room for your question about worms. It seems clear your explanation is right.—W. A. R. Should have put in note, but hoped to publish it ere this.—E. O. J. Thanks. Would insert your answer to Theta's paradox, had we space. A paradox is usually regarded as opposed to what is orthodox.—M. J. H. Impossible to answer your query without breaking through rules.—W. A. C. See reply above to J. H. Symington.—H. A. BULLEY. Will repeat later on your lunar views.—W. G. ROLFE. Alas! no room for reply. It is not a question of "may" and "might," but of "must not" and "cannot."—PHILARIST. You shall keep your "knowledge in a box," we keeping our five shillings "with no further trouble." Seriously, that is not the way we manage.—S. S. S. S. Do not know Cyples' book. We might reply to all queries if we knew how. But knowledge is very wide.—FRESHMAN. The reverend doctor of medicine said much more than he could prove. A man who would say that "not a tittle of evidence could be found for evolution," would say anything.—J. P. F. You rather misunderstand the way in which, according to the theory of natural selection, leopards have become spotted. The case more nearly resembles modern breeding than the Jacobian system you refer to.

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